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[THIRD SERIES.]

ART. XLV.—*The Mathematical Theories of the Earth*; by R. S. WOODWARD, as Vice-president of Section A, of the American Association for the Advancement of Science, Toronto meeting, Aug. 27 to Sept. 3, 1889.

THE name of this section, which, by your courtesy, it is my duty to address to-day, implies a community of interest among astronomers and mathematicians. This community of interest is not difficult to explain. We can of course imagine a considerable body of astronomical facts quite independent of mathematics. We can also imagine a much larger body of mathematical facts quite independent of and isolated from astronomy. But we never think of astronomy in the large sense without recognizing its dependence on mathematics, and we never think of mathematics as a whole without considering its capital applications in astronomy.

Of all the subjects and objects of common interest to us, the earth will easily rank first. The earth furnishes us with a stable foundation for instrumental work and a fixed line of reference, whereby it is possible to make out the orderly arrangement and procession of our solar system and to gain some inkling of other systems which lie within telescopic range. The earth furnishes us with a most attractive store of real problems; its shape, its size, its mass, its precession and nutation, its internal

heat, its earthquakes and volcanoes, and its origin and destiny, are to be classed with the leading questions for astronomical and mathematical research. We must of course recognize the claims of our friends the geologists to that indefinable something called the earth's crust, but, considered in its entirety and in its relations to similar bodies of the universe, the earth has long been the special province of astronomers and mathematicians. Since the times of Galileo and Kepler and Copernicus it has supplied a perennial stimulus to observation and investigation, and it promises to tax the resources of the ablest observers and analysts for some centuries to come. The mere mention of the names of Newton, Bradley, d'Alembert, Laplace, Fourier, Gauss, and Bessel, calls to mind not only a long list of inventions and discoveries but the most important parts of mathematical literature. In its dynamical and physical aspects the earth was to them the principal object of research, and the thoroughness and completeness of their contributions toward an explanation of the "system of the world" are still a source of wonder and admiration to all who take the trouble to examine their works.

A detailed discussion of the known properties of the earth and of the hypotheses concerning the unknown properties, is no fit task for a summer afternoon; the intricacies and delicacies of the subject are suitable only for another season and a special audience. But it has seemed that a somewhat popular review of the state of our mathematical knowledge of the earth might not be without interest to those already familiar with the complex details, and might also help to increase that general interest in science, the promotion of which is one of the most important functions of this association.

As we look back through the light of modern analysis, it seems strange that the successors of Newton, who took up the problem of the shape of the earth, should have divided into hostile camps over the question whether our planet is elongated or flattened at the poles. They agreed in the opinion that the earth is a spheroid, but they debated, investigated, and observed for nearly a half century before deciding that the spheroid is oblate rather than oblong. This was a critical question and its decision marks perhaps the most important epoch in the history of the figure of the earth. The Newtonian view of the oblate form found its ablest supporters in Huyghens, Maupertuis, and Clairaut, while the erroneous view was maintained with great vigor by the justly distinguished Cassinian school of astronomers. Unfortunately for the Cassinians, defective measures of a meridional arc in France gave color to the false theory and furnished one of the most conspicuous instances of the deterring effect of an incorrect observation. As you well know

the point was definitely settled by Maupertuis' measurement of the Lapland arc. For this achievement his name has become famous in literature as well as in science, for his friend Voltaire congratulated him on having "flattened the poles and the Cassinis" and Carlyle has honored him with the title of "Earth-flattener."*

Since the settlement of the question of the form, progress toward a knowledge of the size of the earth has been consistent and steady, until now it may be said that there are few objects with which we have to deal whose dimensions are so well known as the dimensions of the earth. But this is a popular statement, and like most such, needs to be explained in order not to be misunderstood. Both the size and shape of the earth are defined by the lengths of its equatorial and polar axes; and, knowing the fact of the oblate spheroidal form, the lengths of the axes may be found within narrow limits from simple measurements conducted on the surface, quite independently of any knowledge of the interior constitution of the earth. It is evident in fact, without recourse to mathematical details, that the length of any arc, as a degree of latitude or longitude, on the earth's surface, must depend on the lengths of those axes. Conversely, it is plain that the measurement of such an arc on the surface and the determination of its geographical position constitute an indirect measurement of the axes Hence it has happened that scientific as distinguished from practical geodesy has been concerned chiefly with such linear and astronomical measurements, and the zeal with which this work has been pursued is attested by triangulations on every continent. Passing over the earlier determinations as of historical interest only, all of the really trustworthy approximations to the lengths of the axes have been made within the half century just passed. The first to appear of these approximations were the well founded values of Airy, published in 1830.† These, however, were almost wholly overshadowed and supplanted eleven years later by the values of Bessell ‡ whose spheroid came to occupy a most conspicuous place in geodesy for more than a quarter of a century. Knowing as we now do that Bessel's values were considerably in error, it seems not a little remarkable that they should have been so long accepted without serious question. One obvious reason is found in the fact that a considerable lapse of time was essential for the accumulation of new data, but two other possible reasons of a different character are worthy of notice because they are interesting and instruc-

* Todhunter, History of the Theories of Attraction and the Figure of the Earth, London, 1873, Vol. 1, Art. 195.

† Encyclopedia Metropolitana.

‡ Astronomische Nachrichten, No. 438, 1841.

tive whether specially applicable to this particular case or not. It seems not improbable that the close agreement of the values of Airy and Bessel, computed independently and by different methods—the greatest discrepancy being about one hundred and fifty feet,—may have been incautiously interpreted as a comfirmation of Bessel's dimensions, and hence led to their too ready adoption. It seems also not improbable that the weight of Bessel's great name may have been too closely associated in the minds of his followers with the weights of his observations and results. The sanction of eminent authority, especially if there is added to it the stamp of an official seal, is sometimes a serious obstacle to real progress. We cannot do less than accord to Bessel the first place among the astronomers and geodesists of his day, but this is no adequate justification for the exaggerated estimate long entertained of the precision of the elements of his spheroid.

The next step in the approximation was the important one of Clarke in 1866.* His new values showed an increase over Bessel's of about half a mile in the equatorial semi-axis and about three-tenths of a mile in the polar semi-axis. Since 1866, General Clarke has kept pace with the accumulating data and given us so many different elements for our spheroid that it is necessary to affix a date to any of his values we may use. The later values, however, differ but slightly from the earlier ones, so that the spheroid of 1866, which has come to be pretty generally adopted, seems likely to enjoy a justly greater celebrity than that of its immediate predecessor. The probable error of the axes of this spheroid is not much greater than the hundred thousandth part,† and it is not likely that new data will change their lengths by more than a few hundred feet.

In the present state of science, therefore, it may be said that the first order of approximation to the form and dimensions of the earth has been successfully attained. The question which follows naturally and immediately is, how much further can the approximation be carried? The answer to this question is not yet written, and the indications are not favorable for its speedy announcement. The first approximation, as we have seen, requires no knowledge of the interior density and arrangement of the earth's mass; it proceeds on the simple assumption that the sea surface is closely spheroidal. The second approximation, if it be more than a mere interpolation formula, requires a knowledge of both the density and arrangement of the constituents of the earth's mass, and especially of that part called the crust. "All astronomy," says Laplace, "rests on the

* Comparisons of Standards of Length, Made at the Ordinance Office, Southampton, England, by Capt. A. R. Clarke, R. E. Published by order of the Secretary of State for War, 1866.

† Clarke, Col. A. R., Geodesy, Oxford, 1880, p. 319.

stability of the earth's axis of rotation."* In a similar sense we may say all geodesy rests on the direction of the plumb-line. The simple hypothesis of a spheroidal form, assumes that the plumb-line is everywhere coincident with the normal to the spheroid, or that the surface of the spheroid coincides with the level of the sea. But this is not quite correct. The plumb-line is not in general coincident with the normal, and the actual sea-level or geoid must be imagined to be an irregular surface lying partly above and partly below the ideal spheroidal surface. The deviations, it is true, are relatively small, but they are in general much greater than the unavoidable errors of observation and they are the exact numerical expression of our ignorance in this branch of geodesy. It is well known, of course, that deflections of the plumb-line can sometimes be accounted for by visible masses, but on the whole it must be admitted that we possess only the vaguest notions of their cause and a most inadequate knowledge of their distribution and extent.

What is true of plumb-line deflections is about equally true of the deviations of the intensity of gravity from what may be called the spheroidal type. Given a closely spheroidal form of the sea level and it follows from the law of gravitation, as a first approximation, without any knowledge of the distribution of the earth's mass, that the increase of gravity varies as the square of the sine of the latitude in passing from the equator to the poles. This is the remarkable theorem of Stokes,† and it enables us to determine the form, or ellipticity of the earth, by means of pendulum observations alone. It must be admitted, however, that the values for the ellipticity recently obtained in this way by the highest authorities, Clarke‡ and Helmert,§ are far from satisfactory whether we regard them in the light of their discrepany or in the light of the different methods of computing them. In general terms we may say that the difficulty in the way of the use of pendulum observations still hinges on the treatment of local anomalies and on the question of reduction to sea level. At present, the case is one concerning which the doctors agree neither in their diagnosis nor in their remedies.

Turning attention now from the surface toward the interior, what can be said of the earth's mass as a whole, of its laws of

*Toute l'Astronomie repose sur l'invariabilité de l'axe de rotation de la Terre à la surface du sphéroïde terrestre et sur l'uniformité de cette rotation. Mécanique Céleste, Paris, 1882, tome 5, p. 22.

†Stokes, G. G., Mathematical and Physical Papers, Cambridge University Press, 1880, vol. ii.

‡Geodesy, Chapter XIV.

§Helmert, Dr. F. R., Die mathematischen und physikalischen Theorien der höheren Geodäsie, Leipzig, 1880, 1884, II Teil.

distribution, and of the pressures that exist at great depths? Two facts, namely, the mean density and the surface density are roughly known; and a third fact, namely, the precession constant, or the ratio of the difference of the two principal moments of inertia to the greater of them, is known with something like precision. These facts lie within the domain of observation and require only the law of gravitation for their verification. Certain inferences also from these facts and others have long been and still are held to be hardly less cogent and trustworthy, but before stating them it will be well to recall briefly the progress of opinion concerning this general subject during the past century and a half.

The conception of the earth as having been primitively fluid was the prevailing one among mathematicians before Clairaut published his Théorie de la Figure de la Terre in 1743. By the aid of this conception Clairaut proved the celebrated theorem which bears his name, and probably no idea in the mechanics of the earth has been more suggestive and fruitful. It was the central idea in the elaborate investigations of Laplace and received at his hands a development which his successors have found it about equally difficult to displace or to improve. From the idea of fluidity spring naturally the hydrostatical notions of pressure and level surfaces, or the arrangement of fluid masses in strata of uniform density. Hence follows, also, the notion of continuity of increase in density from the surface toward the center of the earth. All of the principal mechanical properties and effects of the earth's mass, viz: the ellipticity, the surface density, the mean density, the precession constant, and the lunar inequalities, were correlated by Laplace in a single hypothesis, involving only one assumption in addition to that of original fluidity and the law of gravitation.* This assumption relates to the compressibility of matter and asserts that the ratio of the increment of pressure to the increment of density is proportional to the density. Many interesting and striking conclusions follow readily from this hypothesis, but the most interesting and important are those relative to density and pressure, especially the latter, whose dominance as a factor in the mechanics of celestial masses seems destined to survive whether the hypothesis stands or falls. The hypothesis requires that while the density increases slowly from something less than 3 at the surface to about 11 at the center of the earth, the pressure within the mass increases rapidly below the surface, reaching a value surpassing the crushing strength of steel at the depth of a few miles and amounting at the center to no less than three million atmospheres. The inferences, then, as distinguished from the

* Mécanique Céleste, Tome 5, Libre xi.

facts, are that the mass of the earth is very nearly symmetrically disposed about its center of gravity, that pressure and density except near the surface are mutually dependent, and that the earth in reaching this stage has passed through the fluid or quasi fluid state.

Later writers have suggested other hypotheses for a continuous distribution of the earth's mass, but none of them can be said to rival the hypothesis of Laplace. Their defects lie either in not postulating a direct connection between density and pressure or in postulating a connection which implies extreme or impossible values for these and other mechanical properties of the mass.

It is clear, from the positiveness of his language in frequent allusions to this conception of the earth, that Laplace was deeply impressed with its essential correctness. "Observations," he says, "prove incontestably that the densities of the strata (couches) of the terrestrial spheroid increase from the surface to the center";* and "the regularity with which the observed variation of a second's pendulum follows the law of the squares of the sines of the latitudes, proves that the strata are arranged symmetrically about the center of gravity of the earth."† The more recent investigations of Stokes, to which allusion has already been made, forbid our entertaining anything like so confident an opinion of the earth's primitive fluidity or of a symmetrical and continuous arrangement of its strata. But, though it must be said that the sufficiency of Laplace's arguments has been seriously impugned, we can hardly think the probability of the correctness of his conclusions has been proportionately diminished.

Suppose, however, that we reject the idea of original fluidity. Would not a rotating mass of the size of the earth assume finally the same aspects and properties presented by our planet? Would not pressure and centrifugal force suffice to bring about a central condensation and a symmetrical arrangement of strata similar at least to that required by the Laplacian hypothesis? Categorical answers to these questions cannot be given at present. But whatever may have been the antecedent condition of the earth's mass the conclusion seems unavoidable that at no great depth the pressure is sufficient to break down the structural characteristics of all known substances, and hence to produce

* Enfin il (Newton) regarde la Terre comme homogène, ce qui est contraire aux observations, que prouvent incontestablement que les densités des couches du sphéroïde terrestre croissent de la surface au centre. Mécanique Céleste, Tome 5, p. 9.

† La régularité avec laquelle la variation observée des longueurs du pendule à secondes suit la loi du carré du sinus de la latitude prouve que ces couches sont disposees régulièrement autour du centre de gravité de la Terre et que leur forme est à peu pres eiliptique et de révolution. Ibid, p. 17.

viscous flow whenever and wherever the stress difference exceeds a certain limit, which cannot be large in comparison with the pressure. Purely observational evidence, also, of a highly affirmative kind in support of this conclusion, is afforded by the remarkable results of Tresca's experiments on the flow of solids and by the abundant proofs in geology of the plastic movements and viscous flow of rocks. With such views and facts in mind the fluid stage, considered indispensable by Laplace, does not appear necessary to the evolution of a planet, even if it reach the extreme refinement of a close fulfilment of some such mathematical law as that of his hypotheses. If, as is here assumed, pressure be the dominant factor in such large masses, the attainment of a stable distribution would be simply a question of time. The fluid mass might take on its normal form in a few days or a few months, whereas the viscous mass might require a few thousand or a few million years.

Some physicists and mathematicians, on the other hand, reject both the idea of the existence of great pressure within the earth's mass and the notion of an approach to continuity in the distribution of density. As representing this side of the question the views of the late M. Roche, who wrote much on the constitution of the earth, are worthy of consideration. He tells us that the very magnitude of the central pressure computed on the hypothesis of fluidity is itself a peremptory objection to that hypothesis.* According to his conception, the strata of the earth from the center outward are substantially self-supporting and unyielding. It does not appear, however, that he had submitted this conception to the test of numbers, for a simple calculation will show that no materials of which we have any knowledge would sustain the stress in such shells or domes. If the crust of the earth were self-supporting its crushing strength would have to be about thirty times that of the best cast steel, or five hundred to one thousand times that of granite. The views of Roche on the distribution of terrestrial densities appear equally extreme†. He prefers to consider the mass as made up of two distinct parts, an outer shell or crust whose thickness is about one-sixth of the earth's radius, and a solid nucleus having little or no central condensation. The nucleus is conceived to be purely metallic and to have about the same density as iron. To account for geological phenomena, he postulates a zone of fusion separating the crust from the nucleus. The whole hypothesis is consistently worked out in conformity with the requirements of the ellipticity, the superficial density, the mean

* Mémoire sur l'Etat Intérieur du Globe Terrestre, par M. Edouard Roche. Mémoires de la section des Sciences de l'Académie des Sciences et Lettres de Montpellier. Montpellier, 1880–1884. Tome x. † Ibid.

density and precession; so that to one who can divest his mind of the notion that pressure and continuity are important factors in the mechanics of such masses, the picture which Roche draws of the constitution of our planet will present nothing incongruous.

In a field so little explored and so inaccessible, though hedged about as we have seen by certain sharply limiting conditions, there is room for a wide range of opinion and for great freedom in the play of hypothesis; and although the preponderance of evidence appears to be in favor of a terrestrial mass in which the reign of pressure is well nigh absolute, we should not be surprised a few decades or centuries hence to find many of our notions on this subject radically defective.

If the problem of the constitution and distribution of the earth's mass is yet an obscure and difficult one after two centuries of observation and investigation can we report any greater degree of success in the treatment of that still older problem of the earth's internal heat, of its origin and effects? Concerning phenomena always so impressive and often so terribly destructive as those intimately connected with the terrestrial store of heat, it is natural that there should be a considerable variety of opinion. The consensus of such opinion, however, has long been in favor of the hypothesis that heat is the active cause of many, and a potent factor in most of the grander phenomena which geologists assign to the earth's crust; and the prevailing interpretation of these phenomena is based on the assumption that our planet is a cooling sphere whose outer shell or crust is constantly cracked and crumpled in adjusting itself to the shrinking nucleus.

The conception that the earth was originally an intensely heated and molten mass appears to have first taken something like definite form in the minds of Leibnitz and Descartes.* But neither of these philosophers was armed with the necessary mathematical equipment to subject this conception to the test of numerical calculation. Indeed, it was not fashionable in their day, any more than it is with some philosophers in ours, to undertake the drudgery of applying the machinery of analysis to the details of an hypothesis. Nearly a century elapsed before an order of intellects capable of dealing with this class of questions appeared. It was reserved for Joseph Fourier to lay the foundation and build a great part of the superstructure of our modern theory of heat diffusion, his avowed desire being to solve the great problem of terrestrial heat. "The question of terrestrial temperatures," he says, "has always

* Protogée, ou De la Formation et des Révolutions du Globe, par Leibnitz, Ouvrage traduite . . . . avec une introduction et des notes par le Dr. Bertrand de Saint Germain, Paris, 1859.

appeared to us one of the grandest objects of cosmological studies, and we have had it constantly in view in establishing the mathematical theory of heat."* This ambition, however, was only partly realized. Probably Fourier underestimated the difficulties of his problem for his most ingenious and industrious successors in the same field have made little progress beyond the limits he attained. But the work he left is a perennial index to his genius. Though quite inadequately appreciated by his contemporaries, the Analytical Theory of Heat which appeared in 1820 is now conceded to be one of the epoch-making books. Indeed, to one who has caught the spirit of the extraordinary analysis which Fourier developed and illustrated by numerous applications in this treatise, it is evident that he opened a field whose resources are still far from being exhausted. A little later Poisson took up the same class of questions and published another great work on the mathematical theory of heat.† Poisson narrowly missed being the foremost mathematician of his day. In originality, in wealth of mathematical resources, and in breadth of grasp of physical principles he was the peer of the ablest of his contemporaries. In lucidity of exposition it would be enough to say that he was a Frenchman, but he seems to have excelled in this peculiarly national trait. His contributions to the theory of heat have been somewhat overshadowed in recent times by the earlier and perhaps more brilliant researches of Fourier, but no student can afford to take up that enticing though difficult theory without the aid of Poisson as well as Fourier.

It is natural, therefore, that we should enquire what opinion these great masters in the mathematics of heat diffusion held concerning the earth's store of heat. I say opinions, for, unhappily, this whole subject is still so largely a matter of opinion that in discussing it one may not inappropriately adopt the famous caution of Marcus Aurelius: "Remember that all is opinion." It does not appear that Fourier reached any definite conclusion on this question, though he seems to have favored the view that the earth in cooling from an earlier state of incandescence reached finally through convection, a condition in which there was a uniform distribution of heat throughout its mass. This is the *consistentior status* of Leibnitz, and it begins with the formation of the earth's crust, if not with the consolidation of the entire mass. It thus affords an initial distribution of heat and an epoch from which analysis may start, and the problem for the mathematician is to assign the

* La question des températures terrestres nous a toujours paru un des plus grands objects des études cosmologiques, et nous l'avions principalement en vue en établissant la théorie mathématique de la chaleur. Annales de Chimie et de Physique, 1824. Tome 27, p. 159.

† Théorie Mathématique de la Chaleur. Paris, 1835.

subsequent distribution of heat and the resulting mechanical effects. But no great amount of reflection is necessary to convince one that the analysis cannot proceed without making a few more assumptions. The assumptions which involve the least difficulty, and which for this reason, partly, have met with most favor, are that the conductivity and thermal capacity of the entire mass remain constant, and that the heat conducted to the surface of the earth passes off by the combined process of radiation, convection, and conduction, without producing any sensible effect on surrounding space. These or similar assumptions must be made before the application of theory can begin. In addition, two data are essential to numerical calculations; namely, the diffusivity, or the ratio of the conductivity of the mass to its thermal capacity, and the initial uniform temperature. The first of these can be observed, approximately, at least; the second can only be estimated at present. With respect to these important points, which must be considered after the adoption of the *consistentior status*, the writings of Fourier afford little light. He was content, perhaps, to invent and develop the exquisite analysis requisite to the treatment of such problems.

Poisson wrote much on the whole subject of terrestrial temperatures and carefully considered most of the troublesome details which lay between his theory and its application. While he admitted the nebular hypothesis and an initial fluid state of the earth, he rejected the notion that the observed increase of underground temperature is due to a primitive store of heat. If the earth was originally fluid by reason of its heat, a supposition which Poisson regarded quite gratuitous, he conceived that it must cool and consolidate from the center outward;* so that according to this view the crust of our planet arrived at a condition of stability only after the supply of heat had been exhausted. But Poisson was not at a loss to account for the observed temperature gradient in the earth's crust. Always fertile in hypotheses, he advaneed the idea that there exist by reason of interstellar radiations, great variations in the temperature of space, some vast regions being comparatively cool and others intensely hot, and that the present store of terrestrial heat was acquired by a journey of the solar system through one of the hotter regions. "Such is," he says, "in my opinion, the true cause of the augmentation of temperature which occurs as we descend below the surface of the globe."† This hypothesis was the result of Poisson's mature reflection, and as such is well worthy of attention. The no-

* Théorie Mathématique de la Chaleur, Supplement de, Paris, 1837.

† "Telle est, dous mon opinion, la cause véritable de l'augmentation de température qui a lieu sur chaque verticale à mesure que l'on s'abaisse au dessous de la surface du globe."—Ibid., p. 15.

tion that there exist hot foci in space was advanced also in another form in 1852 by Rankine, in his interesting speculation on the reconcentration of energy. But whatever we may think of the hypothesis as a whole it does not appear to be adequate to the case of the earth unless we suppose the epoch of transit through the hot region exceedingly remote, and the temperature of that region exceedingly high. The continuity of geological and paleontological phenomena is much better satisfied by the Leibnitzian view of an earth long subject to comparatively constant surface conditions but still active with the energy of its primitive heat.

Notwithstanding the indefatigable and admirable labors of Fourier and Poisson in this field, it must be admitted that they accomplished little more than the preparation of the machinery with which their successors have sought and are still seeking to reap the harvest. The difficulties which lay in their way were not mathematical but physical. Had they been able to make out the true conditions of the earth's store of heat they would undoubtedly have reached a high grade of perfection in the treatment of the problem. The theory as they left it was much in advance of observation, and the labors of their successors have therefore necessarily been directed largely toward the determination of the thermal properties of the earth's crust and mass.

Of those who in the present generation have contributed to our knowledge and stimulated the investigation of this subject it is hardly necessary to say that we owe most to Sir William Thomson. He has made the question of terrestrial temperatures highly attractive and instructive to astronomers and mathematicians, and not less warmly interesting to geologists and paleontologists. Whether we are prepared to accept his conclusions or not, we must all acknowledge our indebtedness to the contributions of his master hand in this field, as well as in most other fields of terrestrial physics. The contribution of special interest to us in this connection is his remarkable memoir on the secular cooling of the earth.* In this memoir he adopts the simple hypothesis of a solid sphere whose thermal properties remain invariable while it cools by conduction from an initial state of uniform temperature, and draws therefrom certain striking limitations on geologic time. Many geologists were startled by these limitations and geologic thought and opinion have since been widely influenced by them. It will be of interest, therefore, to state a little more fully and clearly the grounds from which his arguments proceed. Conceive a sphere having a uniform temperature initially, to cool in a

* Transactions of the Royal Society of Edinburgh, 1862. Thomson and Tait's Natural Philosophy, vol. i, Part II, Appendix D.

medium which instantly dissipates all heat brought by conduction to its surface, thus keeping the surface at a constant temperature. Suppose we have given the initial excess of the sphere's temperature over that of the medium. Suppose also that the capacity of the mass of the sphere for the diffusion of heat is known and known to remain invariable during the process of cooling. This capacity is called diffusivity and is a constant which can be observed. Then from these data the distribution of temperature at any future time can be assigned, and hence also the rate of temperature increase, or the temperature gradient, from the surface toward the center of the sphere can be computed. It is tolerably certain that the heat conducted from the interior to the surface of the earth does not set up any reaction which in any sensible degree retards the process of cooling. It escapes so freely that, for practical purposes, we may say it is instantly dissipated. Hence, if we can assume that the earth had a specified uniform temperature at the initial epoch, and can assume its diffusivity to remain constant, the whole history of cooling is known as soon as we determine the diffusivity and the temperature gradient at any point. Now Sir William Thomson determined a value for the diffusivity from measurements of the seasonal variations of underground temperatures, and numerous observations of the increase of temperature with depth below the earth's surface gave an average value for the temperature gradient. From these elements and from an assumed initial temperature of 7000° Fahr., he infers that geologic time is limited to something between 20,000,000 and 400,000,000 years. He says: "We must allow very wide limits in such an estimate as I have attempted to make, but I think we may with much probability say that the consolidation cannot have taken place less than 20,000,000 years ago, or we should have more underground heat than we actually have, nor more than 400,000,000 years ago, or we should not have so much as the least observed underground increment of temperature. That is to say, I conclude that Leibnitz's epoch of emergence of the *consistentior status* was probably between those dates." These conclusions were announced twenty-seven years ago and were republished without modification in 1883. Recently, also, Professor Tait, reasoning from the same basis, has insisted with equal confidence on cutting down the upper limit of geologic time to some such figures as 10,000,000 or 15,000,000 years * As mathematicians and astronomers we must all confess to a deep interest in these conclusions and the hypothesis from which they flow. They are very important if true. But what are the probabilities? Having been at some pains to look into

* Recent Advances in Physical Science, London, 1876.

this matter, I feel bound to state that, although the hypothesis appears to be the best which can be formulated at present, the odds are against its correctness. Its weak links are the unverified assumptions of an initial uniform temperature and a constant diffusivity. Very likely these are approximations, but of what order we cannot decide. Furthermore, if we accept the hypothesis, the odds appear to be against the present attainment of trustworthy numerical results, since the data for calculation, obtained mostly from observations on continental areas, are far too meager to give satisfactory average values for the entire mass of the earth. In short, this phase of the case seems to stand about where it did twenty years ago, when Huxley warned us that the perfection of our mathematical mill is no guaranty of the quality of the grist, adding that "as the grandest mill will not extract wheat flour from peas-pods, so pages of formulæ will not get a definite result out of loose data."*

When we pass from the restricted domain of quantitative results concerning geologic time to the freer domain of qualitative results of a general character, the contractional theory of the earth may be said to still lead all others, though it seems destined to require more or less modification, if not to be relegated to a place of secondary importance. Old, however as is the notion that the great surface irregularities of the earth are but the outward evidence of a crumpling crust, it is only recently that this notion has been subjected to mathematical analysis on anything like a rational basis. About three years ago Mr. T. Mellard Reade announced the doctrine that the earth's crust from the joint effect of its heat and gravitation should behave in a way somewhat analogous to a bent beam and should possess at a certain depth a "level of no strain," corresponding to the neutral surface in a beam.† Above the level of no strain, according to this doctrine, the strata will be subjected to compression and will undergo crumpling, while below that level the tendency of the strata to crack and part is overcome by pressure which produces what Reade calls "compressive extension," thus keeping the nucleus compact and continuous. A little later the same idea was worked out independently by Mr. Chas. Davison,‡ and it has since received elaborate mathematical treatment at the hands of Darwin,§ Fisher,‖ and

* Geological Reform (The Anniversary Address to the Geological Society for 1869), Lay Sermons, Addresses and Reviews. D. Appleton & Co. New York, 1871.

† Reade, T. Mellard, Origin of Mountain Ranges, London, 1886.

‡ On the Distribution of Strain in the Earth's Crust Resulting from Secular Cooling; with special reference to the growth of continents and the formation of mountain chains. By Charles Davison, with a Note by G. H. Darwin, Philosophical Transactions, vol. clxxviii (1887), A, pp. 231–249. § Ibid.

‖ Fisher, Rev. Osmond, Physics of the Earth's Crust, second edition, London, 1889, Chapter VIII.

others. The doctrine requires for its application a competent theory of cooling and hence cannot be depended on at present to give anything better than a general idea of the mechanics of crumpling and a rough estimate of the magnitudes of the resulting effects. Using Thomson's hypothesis, it appears that the stratum of no strain moves downward from the surface of the earth at a nearly constant rate during the earlier stages of cooling, but more slowly during the later stages; its depth is independent of the initial temperature of the earth; and if we adopt Thomson's value of the diffusivity it will be about two and a third miles below the surface in a hundred million years from the beginning of cooling, and a little more than fourteen miles below the surface in 700,000,000 years. The most important inference from this theory is that the geological effects of secular cooling will be confined for a very long time to a comparatively thin crust. Thus, if the earth is a hundred million years old crumpling should not extend much deeper than two miles. A test to which the theory has been subjected, and one which some* consider crucial against it, is the volumetric amount of crumpling shown by the earth at the present time. This is a difficult quantity to estimate, but it appears to be much greater than the theory can account for.

The opponents of the contractional theory of the earth, believing it quantitatively insufficient, have recently revived and elaborated an idea first suggested by Babbage and Herschel† in explanation of the greater folds and movements of the crust. This idea figures the crust as being in a state bordering on hydrostatic equilibrium which cannot be greatly disturbed without a readjustment and consequent movement of the masses involved. According to this view, the transfer of any considerable load from one area to another is followed sooner or later by a depression over the loaded area and a corresponding elevation over the unloaded one; and in a general way it is inferred that the elevation of continental areas tends to keep pace with erosion. The process by which this balance is maintained has been called isostasy, and the crust is said to be in an isostatic state.‡ The dynamics of the superficial strata with the attendant phenomena of folding and faulting are thus referred to gravitation alone, or to gravitation and whatever opposing force the rigidity of the strata may offer. In a mathematical sense, however, the theory of isostacy is in a less

* Notably Rev. Osmond Fisher. See his Physics of the Earth's Crust, Chapter VIII.

† Appendix to the Ninth Bridgewater Treatise (by C. Babbage), second edition, London, 1838.

‡ Dutton, Capt. C. E., On some of the Greater Problems of Physical Geology, Bulletin Philosophical Society of Washington, vol. xi, pp. 51–64.

satisfactory state than the theory of contraction. As yet we can see only that isostasy is an efficient cause if once set in action; but how it is started and to what extent it is adequate remain to be determined. Moreover, isostasy does not seem to meet the requirements of geological continuity, for it tends rapidly towards stable equilibrium, and the crust ought therefore to reach a state of repose early in geologic time. But there is no evidence that such a state has been attained, and but little if any evidence of diminished activity in crustal movements during recent geologic time. Hence we infer that isostasy is competent only on the supposition that it is kept in action by some other cause tending constantly to disturb the equilibrium which would otherwise result. Such a cause is found in secular contraction, and it is not improbable that these two seemingly divergent theories are really supplementary.

Closely related to the questions of secular contraction and the mechanics of crust movements are those vexed questions of earthquakes, volcanism, the liquidity or solidity of the interior, and the rigidity of the earth's mass as a whole—all questions of the greatest interest, but still lingering on the battle fields of scientific opinion. Many of the "thrice slain" combatants in these contests would fain risk being slain again; and whether our foundation be liquid or solid, or to speak more precisely, whether the earth may not be at once highly plastic under the action of long continued forces and highly rigid under the action of periodic forces of short period, it is pretty certain that some years must elapse before the arguments will be convincing to all concerned. The difficulties appear to be due principally to our profound ignorance of the properties of matter subject to the joint action of great pressure and great heat. The conditions which exist a few miles beneath the surface of the earth are quite beyond the reach of laboratory tests as hitherto developed, but it is not clear how our knowledge is to be improved without resort to experiments of a scale in some degree comparable with the facts to be explained. In the meantime, therefore, we may expect to go on theorizing, adding to the long list of dead theories which mark the progress of scientific thought, with the hope of attaining the truth not so much by direct discovery as by the laborious process of eliminating error.

When we take a more comprehensive view of the problems presented by the earth, and look for light on their solution in theories of cosmogony, the difficulties which beset us are no less numerous and formidable than those encountered along special lines of attack. Much progress has recently been made,

however, in the elaboration of such theories. Roche,* Darwin† and others have done much to remove the nebulosity of Laplace's nebular hypothesis. Poincaré‡ and Darwin§ have gone far toward bridging the gaps which have long rendered the theory of rotating fluid masses incomplete. Poincaré has in fact shown us how a homogeneous rotating mass might, through loss of heat and consequent contraction, pass from the spheroidal form to the Jacobian ellipsoidal form, and thence, by reason of its increasing speed of rotation, separate into two unequal masses. Darwin, starting with a swarm of meteorites and gravitation as a basis has reached many interesting and instructive results in the endeavor to trace out the laws of evolution of a planetary system.‖ But notwithstanding the splendid researches of these and other investigators in this field, it must be said that the real case of the solar system, or of the earth and the moon, still defies analysis; and that the mechanics of the segregation of a planet from the sun, or of a satellite from a planet, if such an event has ever happened, or the mechanics of the evolution of a solar system from a swarm of meteorites, are still far from being clearly made out.

Time does not permit me to make anything but the briefest allusion to the comparatively new science of mathematical meteorology, with its already considerable list of well defined theories pressing for acceptance or rejection. Nor need I say more with reference to those older mathematical questions of the tides and terrestrial magnetism, than that they are still unsettled. These and many other questions, old and new, might serve equally well to illustrate the principal fact this address has been designed to emphasize, namely, that the mathematical theories of the earth already advanced and elaborated are by no means complete, and that no mathematical Alexander need yet pine for other worlds to conquer.

Speculations concerning the course and progress of science are usually untrustworthy if not altogether fallacious. But, being delegated for the hour to speak to and for mathematicians and astronomers, it may be permissible to offer, in closing,

* Essai sur la Constitution et L'Origine du Système Solaire, par M. Édouard Roche. Mémoires de L'Académie des Sciences et Lettres de Montpellier, tome viii, 1873.

† On the Precession of a Viscous Spheroid and on the remote History of the Earth, Phil. Trans., Part II. 1879; On the secular changes in the Elements of the Orbit of a Satellite revolving about a tidally distorted Planet, Phil. Trans., Part II, 1880; On the Tidal Friction of a Planet attended by several Satellites, and on the Evolution of the Solar System, Phil. Trans., Part II, 1881.

‡ Sur L'Équilibre d'une Masse Fluid animé d'un mouvement de rotation, Acta Mathematica, vol vii, 1885.

§ On Figures of Equilibrium of Rotating Masses of Fluid, Phil. Trans., vol. 187, 1887.

‖ On the Mechanical Conditions of a Swarm of Meteorites and on Theories of Cosmogony, Phil. Trans., vol. 180, 1889.

a single suggestion, which will perhaps help us to orient ourselves aright in our various fields of research. If the curve of scientific progress in any domain of thought could be drawn, there is every reason to believe that it would exhibit considerable irregularities. There would be marked maxima and minima in its general tendency toward the limit of perfect knowledge; and it seems not improbable that the curve would show throughout some portions of its length a more or less definitely periodic succession of maxima and minima. Races and communities as well as individuals, the armies in pursuit of truth as well as those in pursuit of plunder, have their periods of culminating activity and their periods of placid repose. It is a curious fact that the history of the mathematical theories of the earth presents some such periodicity. We have the marked maximum of the epoch of Newton near the end of the 17th century, with the equally marked maximum of the epoch of Laplace near the end of the 18th century; and, judging from the recent revival of geodesy and astronomy in Europe, and from the well nigh general activity in mathematical and geological research, we may hope if not expect that the end of the present century will signalize a similar epoch of productive activity. The minima periods which followed the epochs of Newton and Laplace are less definitely marked but not less noteworthy and instructive. They were not the periods of placid repose; to find such one must go back into the night of the middle ages; but they were periods of greatly diminished energy, periods during which those who kept alive the spirit of investigation were almost as conspicuous for their isolation as for their distinguished abilities. Many causes, of course, contributed to produce these minima periods, and it would be an interesting study in philosophic history to trace out the tendency and effect of each cause. It is desired here, however, to call attention to only one cause which contributed to the somewhat general apathy of the periods mentioned, and which always threatens to dampen the ardor of research immediately after the attainment of any marked success or advance. I refer to the impression of contentment with and acquiescence in the results of science, which seems to find easy access to trained as well as untrained minds before an investigation is half completed or even fairly begun. That some such tacit persuasion of the completeness of the knowledge of the earth has at times pervaded scientific thought, there can be no doubt. This was notably the case during the period which followed the remarkable epoch of Laplace. The profound impression of the sufficiency of the brilliant discoveries and advances of that epoch is aptly described by Carlyle in the half humorous, half sarcastic language of Sartor Re-

sartus. "Our Theory of Gravitation," he says, "is as good as perfect: Lagrange, it is well known, has proved that the Planetary System, on this scheme, will endure forever; Laplace, still more cunningly, even guesses that it could not have been made on any other scheme. Whereby, at least, our nautical logbooks can be better kept; and water transport of all kinds has grown more commodious. Of geology and geognosy we know enough; what with the labors of our Werners and Huttons, what with the ardent genius of their disciples, it has come about that now, to many a royal society, the creation of a world is little more mysterious than the cooking of a dumpling, concerning which last, indeed, there have been minds to whom the question *How the apples were got in?* presented difficulties." This was written nearly sixty years ago, about the time the sage of Ecclefechan abandoned his mathematics and astronomy for literature to become the seer of Chelsea; but the force of its irony is still applicable, for we have yet to learn, essentially, "*How the apples were got in*," and what kind they are.

As to the future we can only guess, less or more vaguely, from our experience in the past and from our knowledge of present needs. Though the dawn of that future is certainly not heralded by rosy tints of over-confidence amongst those acquainted with the difficulties to be overcome, the prospect on the whole has never been more promising. The converging lights of many lines of investigation are now brought to bear on the problems presented by our planet. There is ample reason to suppose that our day will witness a fair average of those happy accidents in science which lead to the discovery of new principles and new methods. We have much to expect from the elaborate machinery and perfected methods of the older and more exact sciences of measuring and weighing—astronomy, geodesy, physics and chemistry. We have more to expect, perhaps, from geology and meteorology with their vast accumulations of facts not yet fully correlated. Much, also, may be anticipated from that new astronomy which looks for the secrets of the earth's origin and history in nebulous masses or in swarms of meteorites. We have the encouraging stimulus of a very general and rapidly growing popular concern in the objects of our enquiries, and the freest avenues for the dissemination of new information; so that we may easily gain the advantage of a concentration of energy without centralization of personal interests. To those, therefore, who can bring the prerequisites of endless patience and unflagging industry, who can bear alike the remorseless discipline of repeated failure and the prosperity of partial success, the field is as wide and as inviting as it ever was to a Newton or a Laplace.

ART. XLVI.—*Darkened Silver Chloride not an Oxychloride;* by M. CAREY LEA.

ABOUT two years ago, I published a series of papers in this Journal, the main object of which may be briefly stated as follows:—to prove that the substances which I described as "photosalts" and obtained by purely chemical means were identical with those produced by light, with both the visibly darkened substances, and the material of the latent image. Further, that all these substances consisted of a silver haloid (normal chloride, bromide or iodide) combined with the corresponding subsalt, not in equivalent proportions, but after the manner of a "lake" The subsalts, being unstable substances when isolated, acquiring much greater stability by the union.

The only objections I have seen to these views were based on investigations made in England by Dr. Hodgkinson: his conclusions were that an oxysalt and not a subsalt was formed. Although several years have elapsed since these conclusions were made public, the means by which they were reached and the necessary experimental proof, do not seem to have been published. Mr. Meldola in his interesting "Chemistry of Photography," in treating of this part of the subject seems disposed to accept Dr. Hodgkinson's theory and his formula Ag_4OCl_2 for darkened silver chloride. Mr. Meldola adopts my views that the photosalts which I described and which were obtained by purely chemical means are identical with the products resulting from the action of light on the silver haloids, but expresses the opinion that I have not proved my theory of their constitution.

It has always seemed to me that the whole mass of observation on the action of light on silver chloride tended so thoroughly to indicate the formation of subchloride, that we might reasonably accept that view, at least until something in the way of proof were offered for the oxychloride theory. But, waiving this objection, I will endeavor to show that subchloride and not oxychloride is the product of the action of light on silver chloride.

The question as to the presence or absence of oxygen in colored silver chloride is one that cannot be determined satisfactorily by quantitative analysis. Taking for example the formula just mentioned, Ag_4OCl_2, it would involve the presence of about 3 per cent of oxygen, if the entire mass of silver chloride were converted into this substance. But we know that even by the longest exposure, the proportion of AgCl acted upon is very small. It would probably be a liberal estimate if

we were to fix 5 per cent of the whole mass as the proportion changed by light. So that the amount of oxygen that would, according to the oxychloride theory, be introduced into a given quantity of chloride by prolonged exposure would not exceed three twentieths or 0·15 of one per cent, of the material under examination, a dangerously small quantity on which to attempt to decide an important question, especially where the estimation is indirect. If even a very careful determination of the silver and the chlorine present should bring the sum of these to a quantity represented by figures amounting to from 99·80 to 99·90, would it be allowable to assume that the difference between this and 100 consisted of oxygen and so to take the presence of an oxychloride as proved? Such reasoning could not be accepted; the errors incident to the most careful analysis would too largely affect the point vitally at issue, not to speak of the entire absence of proof that the deficient quantity was oxygen.

These considerations convinced me that it was not in that direction that one should seek for proof of the presence or absence of oxygen in the the substance in question. I therefore looked for what may be called *proof by exclusion.*

Coal naphtha (refined petroleum) is a substance absolutely free from suspicion of containing oxygen or moisture as impurities, so much so as to be universally used for the preservation of sodium. I have sodium that has been preserved in this way for over thirty years. Silver chloride was precipitated with excess of hydrochloric acid, was washed in a darkened room and dried in a dessicator. From this it was transferred to a porcelain crucible, covered and fused over a lamp. When thoroughly fused (in this condition it is as fluid as water) it was poured directly into naphtha. This naphtha had been placed to the depth of an inch or more in a dry porcelain vessel, which was first well wiped out with naphtha to remove the film of atmospheric moisture which condenses on surfaces.

The chloride congealed into a pale gray lump which, whilst it remained under the petroleum, was absolutely free from all possibility of contact with oxygen, free or combined. Without removing it, the vessel was moved into the sunshine. When touched by sunlight, the chloride instantly became as black as ink.

This experiment seems decisive as to the oxychloride theory.

It seemed desirable not to stop here, but to find a means of applying an equally decisive proof by exclusion to the converse case. In the above instance, a photosalt was formed by reduction starting with normal chloride. The converse case would be the formation of a photosalt by chlorination, starting with

metallic silver, and excluding oxygen, free or combined, thus demonstrating that that element is not needed and plays no necessary part in the formation of silver photochloride.

Anhydrous cupric chloride, which I at first thought of employing, proved to be insoluble in naphtha. In rendering ferric chloride anhydrous there is a possibility of forming oxychloride; it could therefore not be employed. As it is not in the least important which halogen is used, I concluded to take iodine which proved to be slightly soluble in naphtha with a beautiful violet coloration. The mode of operation was as follows. Pure silver reduced by cadmium from the chloride was heated nearly to redness in a porcelain capsule, and at the instant of removal from the flame, was dropped into naphtha. Some fragments of iodine were added. Owing to the very small amount of iodine soluble in naphtha, the action was slow, but continuous and regular. As fast as the iodine dissolved, it was taken up by the silver. At the end of some hours the iodine had disappeared wholly, and the naphtha was colorless. Fresh naphtha replacing it failed to dissolve any iodine. The whole of it had combined with the silver to a black compound. This experiment may be varied by using a piece of clean silver foil or even a silver coin that has been boiled a few moments with nitric acid, washed and heated by a blast lamp. Immersed in the naphtha with iodine its surface soon becomes perfectly black.

This reaction forms the complement of the other and the two show that whether we start from silver chloride and proceed by reduction or from metallic silver and proceed by iodization, in either case we can obtain a photosalt under conditions which rigorously exclude all possibility of the presence of moisture or of oxygen in any shape.

Therefore the photosalt is not an oxysalt, but, as I endeavored to prove two years ago, a compound of normal salt with subsalt.

The action of light upon silver chloride appears to take place in the following manner.

If any substance is present with which chlorine can combine, either directly or by substitution,* the AgCl is decomposed with

* As to the action of light on silver chloride perfectly isolated, i. e. in a perfect vacuum would appear from an interesting experiment of Abney's that no decomposition takes place. It was found that AgCl in vacuo did not darken even by prolonged exposure.

This experiment does not indicate that the presence of moisture is essential for decomposition, it simply proves that some substance (by no means necessarily water) must be present upon which chlorine can act Accordingly, when the vacuum tube contained mercury, the AgCl was decomposed by exposure to light. —It may be remarked that this last mentioned fact, properly considered, would have been found to be fatal to the oxychloride theory, inasmuch as darkened chloride was formed in the total absence of oxygen.

formation of subchloride. As the product is subchloride and not oxychloride it is not necessary that the substances present should contain oxygen, as has just been shown. The subchloride thus formed instantly combines with a portion of the silver chloride as yet unacted upon by light, forming a photochloride of great stability, capable for a time of resisting the action of nitric acid (I have shown that the photochloride made by purely chemical means also shows this stability.)

This combination is not by equivalents but it is of the nature of a lake, and the affinity of silver chloride for the subsalt is of a progressively diminishing character. Small quantities of subchloride are held with great tenacity; as the proportion of subchloride increases the affinity diminishes. This is no assumption, it is easy to form chemical photochloride containing a large proportion of subchloride. Much of this latter is instantly decomposed by cold nitric acid; with heat, an additional quantity disappears and so on until the last portions may require hours of boiling with strong acid, for decomposition.

This stable combination of the chloride and subchloride constitutes alike the material of darkened chloride, of the latent image, and of the photochloride.

An excellent mode of testing the value of a chemical theory is to observe its ability to explain not only the general result of a reaction, but also the secondary facts observable. In the present matter, the action of light on silver chloride, there are two such secondary facts of a quite remarkable nature, for which, though long familiarly known, no explanation has hitherto been found, but which, I think, will be found to be readily and satisfactorily explained by the photochloride theory.

1. When silver chloride is exposed to light, there is a certain pause, an interval during which very little action takes place. After this, the darkening sets in rapidly. This fact is so conspicuous as to attract the attention of everyone who exposes chloride paper.

The explanation is: light pink or violet photochloride is vastly more sensitive to light than white chloride, a fact easily proved by preparing light colored photochloride by any of the chemical means I have elsewhere described, brushing it and white chloride, each in a pasty condition, over respective strips of paper and exposing side by side; the difference in the darkening is very striking.—Now the first action of light on white chloride is to form this light-colored photochloride, and whilst that is going on but little visible effect is produced. As soon as the photochloride is once formed, the darkening becomes rapid. The time required for the first formation of the photochloride gives rise to the pause which is observable.

2. When pure moist silver chloride is exposed to light, the darkening goes on steadily to a certain point and then virtually ceases. Although the mass may be constantly agitated so as to expose fresh portions whilst it is kept moist, or better, kept under water, the action of light after a few days exposure, ends almost wholly, and though the chloride now seems perfectly black, analysis shows that the amount of chloride altered has been very small, perhaps a twentieth. For this well known and most remarkable fact I believe no explanation has ever been offered.

It is to be explained I think as follows: When light acts on moist silver chloride, acid products are formed, the nature of which has not been fully made out, but whose presence is certain. (If the exposure is made under water, the water reddens litmus, it does not bleach it.) These acid products would instantly destroy silver subchloride isolated. As long as the quantity of subchloride present and combined with normal chloride is small, the protecting power of the normal chloride saves the subchloride from decomposition. The ability of normal chloride to protect the subsalt rapidly diminishes as already mentioned as the proportion of the latter increases; these forces in time find an equilibrium (as in the case of many other chemical reactions), in which the quantity of subchloride becomes constant, and any excess that is formed by light beyond the quantity which the chloride can protect is instantly destroyed. Consequently even the most protracted exposure fails to increase the proportion of subchloride beyond its limit.

The complete cessation of the reducing action of light after a certain very small fraction of the chloride present has been attacked cannot be explained by the oxysalt theory. For, if a small portion of the chloride has been converted into Ag_4OCl_2, there is no conceivable reason why a continuance of the same agencies at work should not affect the residue and so go on until the whole of the silver chloride passes into the new condition.—No theory deserves serious consideration that does not offer a satisfactory explanation of these two conspicuous facts: the pause at the outset, and the cessation of action as soon as a certain small proportion of chloride is reduced.

A confirmation of the explanation here offered is to be found in the fact that black forms of photochloride chemically formed are almost wholly unacted upon by light. By treating metallic silver in a state of fine division* with sodium hypochlorite, black photochloride is easily formed which, after two

* For such purposes, metallic silver is best obtained by precipitating the nitrate with sodium hydroxide in excess, and without washing, adding a solution of milk sugar. In a few hours the reaction is complete (Levol's method.) If the presence of any undecomposed oxide is feared the product may be washed with ammonia.

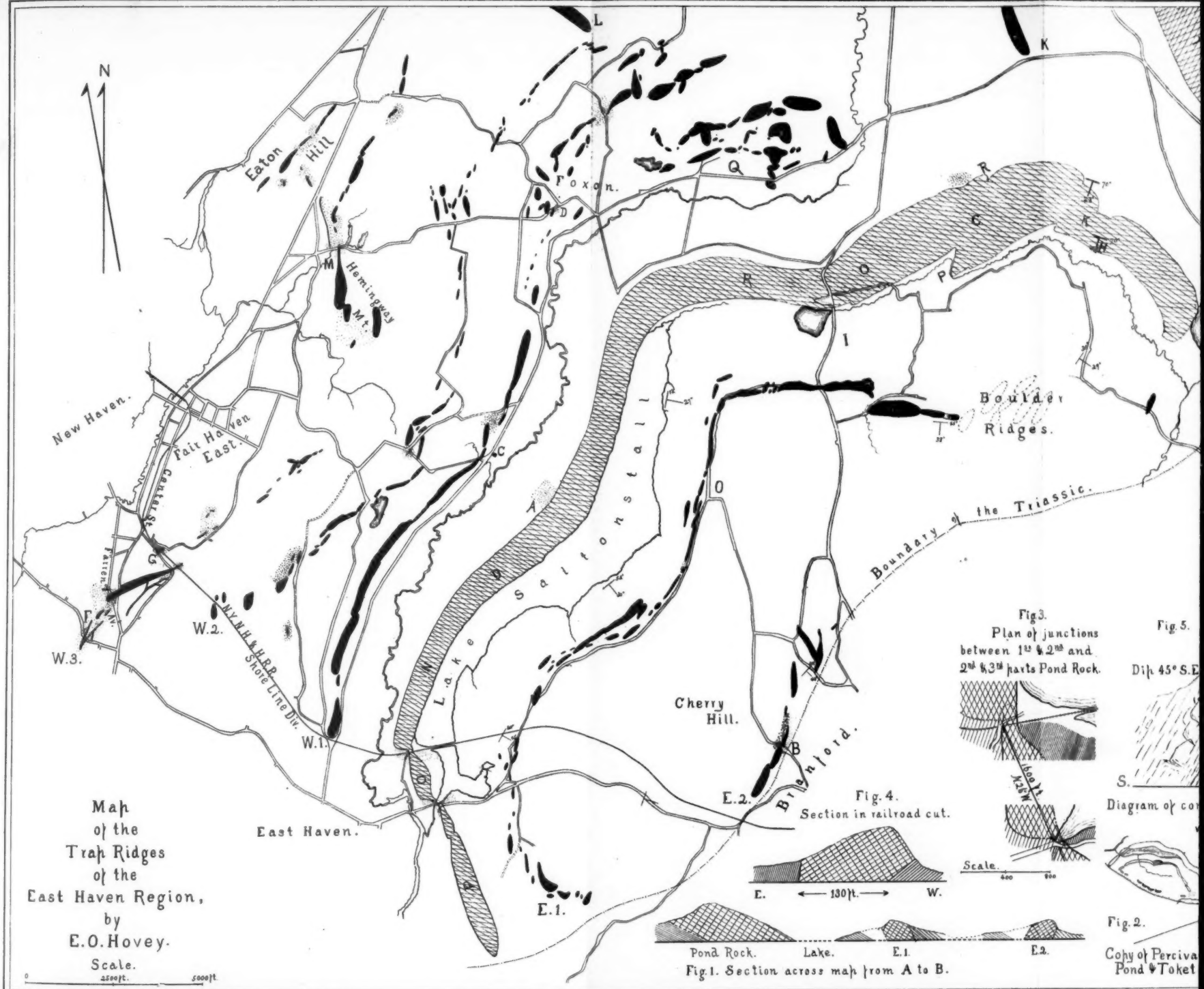


Fig. 3.
Plan of junctions between 1st & 2nd and 2nd & 3rd parts Pond Rock.

Fig. 5.

Diagram of co

Fig. 4.
Section in railroad cut.

Fig. 1. Section across map from A to B.

Fig. 2.

Copy of Perciva
Pond & Toket

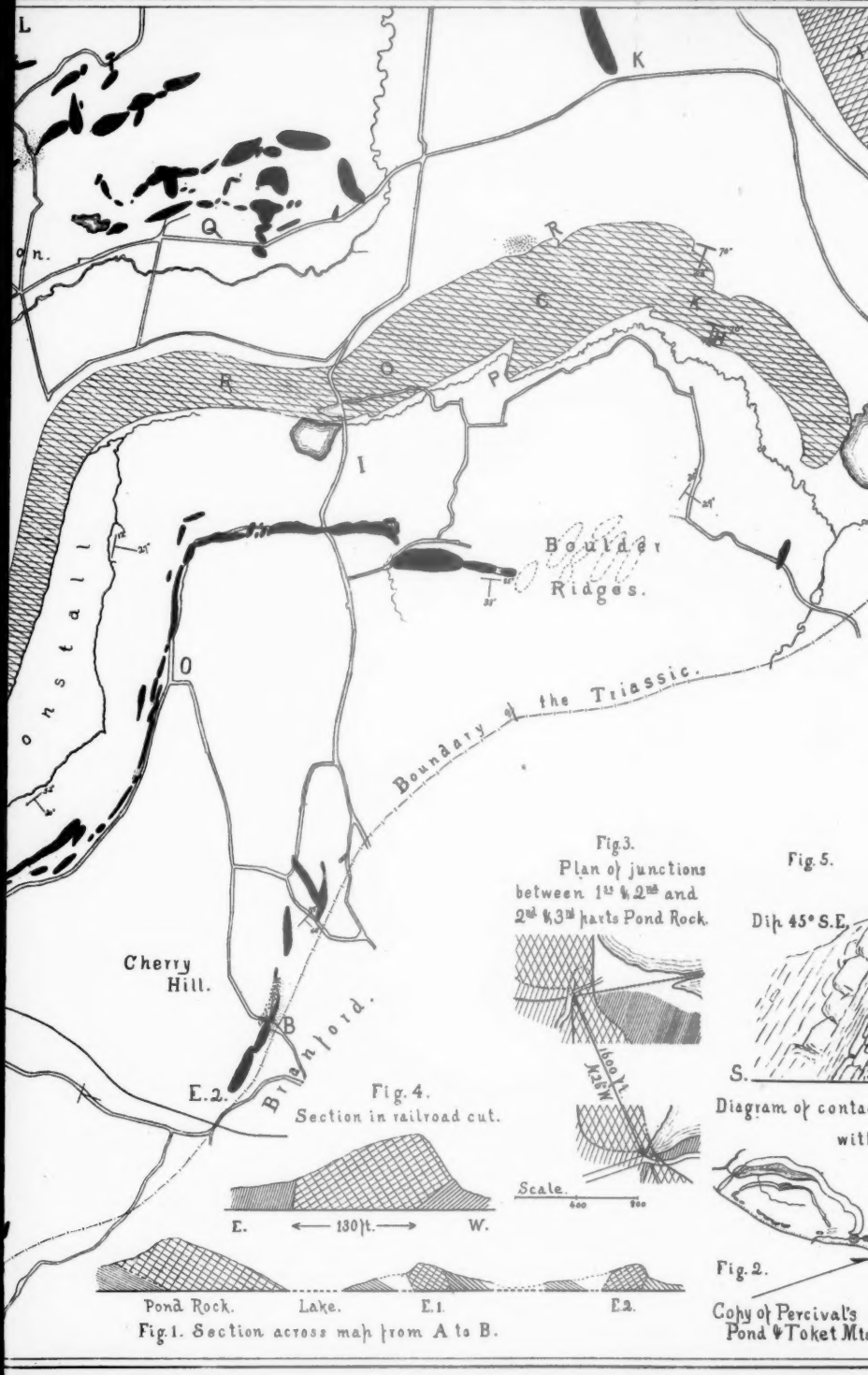


Fig.1. Section across map from A to B.

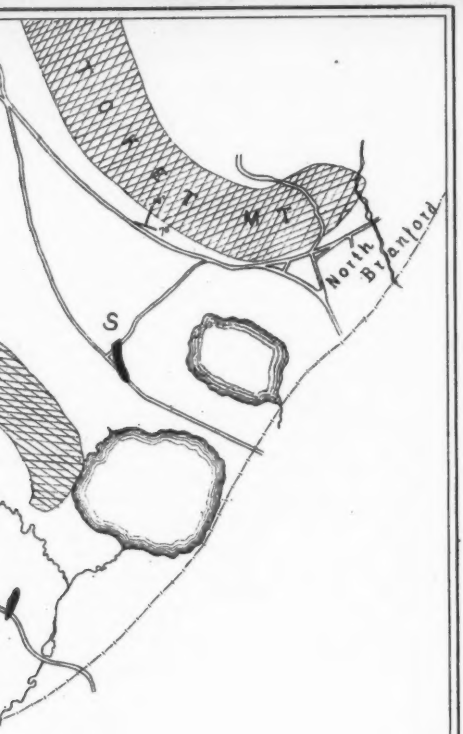
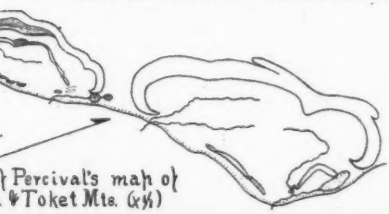


Fig. 5.

am of contact of the trap of the range E.2
with the eastern sandstone at B.



Percival's map of
Toket Mts. (x½)

or three treatments with hypochlorite solution, gives up nothing to cold nitric acid. It is almost wholly insensitive to light; because as fast as normal chloride is decomposed by light it is reconstituted by the conversion of a corresponding quantity of subchloride present, to normal chloride. The chloride and subchloride are in an equilibrium which the action of light does not alter.

August, 1889.

ART. XLVII.—*Observations on some of the Trap Ridges of the East Haven-Branford Region,* with a map* (Plate IX); by EDMUND OTIS HOVEY, Ph.D.

Contents.—The topography of the region—its trap belts and sandstone ridges—general position and forms of the trap belts—particular description of Pond Rock and the ridges east of it. Kinds of trap rock in different parts of the region—the amygdaloidal trap and its relation to that which is non-vesicular. The relation of the sandstone to the trap—contact phenomena. Main theories which have been advanced to account for the occurrence of the trap—special discussion of the theory of "contemporaneous overflow." The age of the trap. Conclusions.

THE character and age of the ridges of trap which occur so numerously in the Triassic sandstone of Massachusetts, Connecticut and New Jersey are questions that have been discussed with more or less vigor for the last seventy years. In the Bulletin of the Museum of Comparative Zoology, 1883, Prof. W. M. Davis has given a valuable list of papers bearing upon the subject. In this Bulletin, in this Journal, III, xxiv, 1882, and III, xxxii, 1886, and in the Seventh Annual Report, U. S. G. S., 1885–86, just issued, he has detailed his own observations, illustrating them by figures and sections, and has elaborated his conclusions as to the trap. The investigations, the results of which are given in the present communication, were undertaken by the writer in the hope of throwing additional light upon the perplexing question.

The region most carefully studied in this connection lies to the east and southeast of the city of New Haven, Conn., in the towns of New Haven, East Haven, Branford and North Branford, and though small, being about eight miles in greatest length, from southwest to northeast, by four miles in width, it is full of interest topographically as well as geologically. The general monoclinal eastward dip of the sandstone, with numerous faults, gives long ridges whose general trend is that of the strike of the strata; i. e, N. 10° or 15° E. Most of these

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ridges present bluffs or very steep faces to the west, while their eastern sides are gradual in slope. These ridges are all low, none of them rising to a height of more than 140 feet above the sea, though in Branford the sandstone rises gradually to the height of 226 feet.*

The most striking feature of the topography, however, is that of the outcrops of trap. These arrange themselves in about six lines of ridges, as is well shown on Percival's elaborate and excellent map of the State,† and also on the map accompanying this article. The most important range comprises Pond Rock (Percival's E. I.), and Totoket Mountain (E. II.), which seem to belong to different systems; while the five subordinate ranges are made up of many small exposures of trap, and lie two to the east and three to the west of Pond Rock. With the exception of the easternmost, these ranges show a general convexity toward the west, while Pond Rock and Totoket are almost semicircles. The eastern of the six ranges is very short, and is remarkable for being convex toward the east—the only well-marked example of the kind in the Connecticut Triassic area.

The map accompanying this article has been prepared with considerable care. Most of it was taken from the charts of the New Haven Region by the U. S. Coast and Geodetic Survey; but the region east of the road crossing Pond Rock, northeast of Lake Saltonstall, and the southern hook of Totoket Mountain were mapped by myself, using a compass and measuring the distances by pacing. The town maps of the district as given in an atlas of New Haven County were found to be almost entirely valueless, even for the roads. The outlines of the exposures of trap were all laid down from my own observations. I have endeavored to represent with approximate accuracy the position and shape of every exposure of trap in the region mapped; the task required the location of about 270 exposures.‡ See further, the closing note on page 383.

Pond Rock.—Pond Rock, with the eastern ridges, is considered by Prof. Davis to contain the key to the whole problem of the origin of the trap ridges of the valley, hence a detailed description of this and the other ranges will not be out of place. The main range, Pond Rock, begins $1\frac{1}{8}$ miles S. 10° E. of the railroad embankment across Lake Saltonstall, and extends northerly and easterly for six miles. For rather more

* Map of the New Haven region by the U. S. Coast Survey.

† Report on the Geology of Connecticut, 1842; J. G. Percival.

‡ The degree of excellence attained by Percival in his map is something marvelous. Working alone and without any of the modern appliances, he produced a map which to-day stands very fairly the test of field work in a region of exceptional intricacy. The scale of his map is small, being but one inch to five and one-half miles.

than three miles the eastern slope of the ridge is washed by the lake, which is noted for the purity and depth of its waters. Three-quarters of a mile from the northern end of the lake and 300 feet from the western shore the Coast Survey found a depth of 107 feet, and other depths nearly as great are recorded. The surface of the lake is less than 15 feet above tide-level, a height which is largely due to a dam. There are three main curves in the range, with the convexity to the west and northwest. The first extends from the southern end of the ridge to the middle of the lake; the second, from the latter point to the crossing of the Branford-Foxon road; the third, from that point to the eastern extremity of the ridge. The whole ridge forms a grand curve, convex toward the northwest, with its chord, $4\frac{3}{4}$ miles long, extending nearly northeast and southwest. From its southern point to the railroad the ridge trends N. 10° W.; north of the head of the lake the trend for two miles is nearly east and west, while the eastern extremity bends around to the south.

This range consists of three distinct parts. The southern portion has a general course of about N. 15° W., is rather more than three-fourths of a mile long, and its highest points are 140 feet above mean tide-level. At its southern end the trap rises close to a ledge of granite from which it is separated by low marshy land. Toward the north the trap tapers out and the ridge decreases in height so that where it crosses the New Haven-Branford turnpike the width of its horizontal section is less than fifty feet, and it rises but thirty feet above the sea. Here the rock is broken into small angular fragments clinging loosely together, and is much decomposed. This is what Prof. Davis calls trap "breccia" or a "brecciated structure," but the term seems to be misapplied; for the rock evidently is not sedimentary in origin, and its structure is due to lines of fracture in the mass which have been made prominent by decomposition. This portion of the ridge ends a few feet north of the turnpike and is now separated from the second part by the extremity of the lake, the waters of which conceal the junction of the two parts. The second part begins about ten feet north of the turnpike, at the dam, and extends somewhat more than a quarter of a mile west of north to a point fifty or sixty feet north of the railroad (the Shore Line Division of the N. Y., N. H. & H. road), which makes a cut thirty feet deep through the ridge. The hill rises to the height of 140 feet, and is surmounted by the stand-pipe of the New Haven Water Co.'s works. This part, like the first, is precipitous toward the west, and slopes more gradually toward the east.

The third part of Pond Rock joins the second by a narrow neck of trap, and, beginning forty or fifty feet north of the

railroad, extends in a bold curve to the northward and eastward for nearly five miles. At the southern end the ridge rises very rapidly to a height of 175 feet, then more gradually until it reaches 240 feet, at a mile and a quarter from the railroad. This height is maintained very uniformly for about three miles, though several points are twenty to thirty feet higher and lower, and two points attain 280 feet of elevation—one at the head of the lake and the other a mile farther east. The eastern extremity of the ridge is a bluff rising 160 feet (aneroid measurement), above a small lake at its base, which is perhaps fifty feet above the sea. The slope of the eastern side of the ridge along Saltonstall lake is usually 25° or 30°, while that of the western side varies from 40° and more at the southern extremity, and at the bend near the head of the lake to 10° or less, in the curve opposite the middle of the lake; east of the head of the lake the southern side is usually steeper than the northern. The *bluffs*, however, are on the convex side. In many places the convex side of the ridge is covered with a heavy talus of trap, while the concave side has comparatively few fragments on it.

It is hard to determine how much of the ridge is trap and how much sandstone. The eastern and southern slopes of the third part of the ridge seem to be composed entirely of trap, except at one point within a mile of the eastern extremity, where sandstone is exposed high up on the slope. Along the lake most of the rock is solid and hard, but much of it is amygdaloidal in structure, indicating that the present may be nearly the same as the original surface. Shale is exposed on the eastern slope of the second part of the ridge, but neither sandstone nor shale has been observed on the same slope along the first part except at the foot of the lake. The western faces of the first and second parts show well the contact of the trap and underlying sandstone, but the dip of the strata is hard to obtain satisfactorily. On the western and northern slopes of the third part of the ridge no contact has been found. Near the railroad the sandstone rises on the ridge to perhaps eighty feet above sea-level. Measuring in the direction of the dip to the same level on the other side of the ridge, as given on the chart of the Coast Survey (an unsatisfactory method), and taking 40° as the dip of the sandstone at this place, we get 225 feet as the thickness of the sheet of trap. Southeast of Lyman Granniss's house (C, on the map), and a mile and a half from the railroad, a trench about 250 feet long was dug into the side of the hill about thirty years ago in a fruitless search for silver. The trench passes through fine, soft shale and ends against fine-grained drab sandstone, which looks as if it had been subjected to the action of heat, and is probably very near the

contact. This sandstone occurs 180 feet above tide-water, aneroid measurement. The locality is marked A on the map. The strike and dip of the sandstone were not obtained here, but taking the dip as 25°, which is probably not too low for this region, and applying the method of calculation just given, the thickness of the trap sheet at this point is about 317 feet. On the same basis the second part of the ridge at the standpipe would have the trap 260 feet thick, and the trap of the first part would be about as thick.

In the village of North Branford, about a mile northeast from the eastern extremity of the Pond ridge, Toket (or Totoket) mountain begins. This trap sheet has the same general outlines as Pond Rock, but it is broader and longer and rises to greater elevations. Its chord trends nearly north-northeast, and is about eight miles in length. The ends of this ridge, like those of the other, rise close to the crystalline rocks which form the eastern boundary of the Triassic region. Only the southern portion of Toket will be especially referred to in the course of the present discussion. This part extends west-northwest for about a mile, being nearly parallel to the eastern extension of Pond Rock. It is higher than the latter and presents a bold, partly precipitous front to the south, while it slopes away very gradually to the north. Percival's map shows such a remarkable "hook" at the southern end of Toket, that especial care was taken in tracing out the shape of the trap, with the result given on my map, which is confirmatory of Percival's outline

The eastern subordinate ranges.—Percival's map and description* of his "first posterior" range (the one next east of Pond Rock and marked E 1, on my map), would indicate that it was almost as continuous as the main ridge, but inspection of the field shows that it is composed of a good many more or less distinct parts. The range begins about a third of a mile east-northeast from the southern end of the Pond ridge, and is remarkable for the close parallelism between it and the main range, not only in the principal curve but also in the subordinate ones. At its southern end two small ridges extend northeasterly. The second eastern range of trap (E. 2), begins about 600 feet north of the railroad in that part of Branford known as Plantsville, and extends N. 30° E. for rather more than a mile, and, as has been already stated, is remarkable for its convexity toward the east. The range consists of seven or eight ridges, three of which are more than 100 feet high, and with E 1 makes an almost complete oval. It is separated from the crystalline rocks by a narrow swampy valley.

* Geology of Connecticut, p. 324.

The western subordinate ranges.—Of the three ranges of trap ridges mentioned as lying west of Pond Rock, the one nearest to the main ridge is the most continuous. Beginning at the railroad, a third of a mile west of the Pond ridge, this range (W 1, on the map), extends to the north and northeast for rather more than four miles. The convexity toward the west is marked, though not as noticeable as that of the main ridge; like that of the Pond ridge at the southern end, it is caused by the exposures of trap being arranged in what Percival calls the advancing order; i. e., the southern end of the northern member overlaps on the west the northern end of the southern member. The northern end of the range consists of a very complex set of short ridges which may, perhaps, be joined into two or three strongly curved ranges. Each of the other two ranges (W 2, and W 3), is composed of many small exposures, but the most western has several large ridges, the largest of which are Hemingway mountain, 260 feet high, and Eaton Hill, 200 feet high, north of the borough of Fair Haven East, and two nameless hills, each 240 feet in height, north of and in line with Hemingway mountain. Besides these ranges of ridges there are many small exposures of trap in this part of the district which do not seem to belong to any range.

Kinds of trap rock in different parts of the region.—Dr. G. W. Hawes* and Prof. E. S. Dana† made a somewhat extended chemical and microscopic study of the trap rocks of the Connecticut valley some years ago. The results, though incomplete, show that the trap is of very uniform mineral composition, and that the main differences are due to the alteration consequent upon hydration. These observers concluded that the hydration increased from west to east, the trap of the western border of the Triassic being almost non-hydrous. While the increase is not a regular one, it is true that the ridges of the eastern part of the valley generally show more alteration than those of the western. The trap is a true diabase, the augite of which has suffered the most from alteration. The alteration products, according to my study of slices, are usually chlorite and magnetite, the latter sometimes arranging itself in beautiful arborescent groups of crystals, while the former frequently exhibits a fibrous structure. The ridge which shows the most complete alteration of any in the region is a part of the most western of the six ranges (W 3). Where exposed by Farren avenue and Center street the rock has been deeply decomposed, while other ridges in its vicinity and Hemingway mountain, a mile and a half north in the same range, are composed of hard dense trap, not very lustrous but still showing some of the augite unaltered. The trap of the

* This Journal, III, ix, 192, 1875. † This Journal, III, viii, 390, 1874.

range next west of the main range contains many amygdules and pipestems of calcite at its southern end and the rock is a dull black; toward the north some of the ridges consist of hard and somewhat lustrous trap, and others of that which is much decomposed and soft; some are amygdaloidal while others are not. These ranges are composed of many isolated members, and some variation might be expected.

The main ridge is composed for the most part of a continuous sheet of trap, but the external appearance and texture of the rock vary somewhat in different parts of it. By far the largest part of the sheet is composed of hard, dense, tough trap which is almost as lustrous as that from East Rock, but its color is a lighter green and planes of alteration penetrate its mass. At the railroad cut the rock is somewhat amygdaloidal, and much of it is decomposed, some even into coarse dark-brown earth The solid rock at this place is green or black, is almost lusterless, and rarely if ever shows its crystalline structure macroscopically.

Along the lake, as has been remarked already, much amygdaloid occurs; it alternates with the dense and the decomposed rock. The amygdules are mostly of calcite, which easily weathers out leaving the rock very vesicular. Fragments of this vesicular trap cover large areas of the southern slope near the eastern extremity of the ridge.

In Prof. E. S. Dana's collection of thin sections there are two from Pond Rock, one from the railroad cut and one from the head of the lake; in mine there are three: one from the lower contact with the sandstone in the railroad cut, one from near the middle of the sheet on the northern side, a mile and a half from the lake, the third from the southern slope within half a mile from the eastern extremity of the ridge, showing the contact with the overlying sandstone. Prof. Dana's sections and the first and third of mine (Nos. 21*a* and 18 of the collection), show very much the same thing, viz: an altered diabase with the augite constituent almost entirely gone, and with the plagioclase somewhat affected. The section (No. 23), from the northern slope is very different from these. Considerable chlorite appears, but much of the augite is apparently but little altered, giving the usual brownish color in ordinary light and bright colors between crossed Nicols. The structure is rather coarsely crystalline and the individuals of the plagioclase twins are much larger than have been observed in other sections. In this section the feldspar constituent seems to be the one that has suffered most from hydration.

No thin sections have been made of the trap of the first range east of Pond Rock, but I have two (Nos. 15 and 25 in my collection), from the second range; the former is from the con-

tact with the eastern sandstone, and the latter is from near the middle of the ridge, and both are from the cut made by the wagon road from Branford to Cherry Hill (at B on the map). No. 25 is of a very finely crystalline rock, hard and lustrous, and breaking with conchoidal fracture. Under the microscope the rock shows less alteration than No. 23. The augite has suffered the most from hydration, though many crystals show their characteristic colors and cleavage lines, and very little chlorite has been formed. The feldspar shows very little alteration; it occurs in its usual form—thin lath-shaped twin crystals, each of which is made up of but two individuals. Much magnetite appears, some of which is arborescent in form, the octahedral crystals being connected along their cubic axes. The trap of No. 15 from the contact shows more alteration than the section just described, the augite being entirely decomposed and more chlorite appearing, the feldspar also has suffered from alteration. Of the other members of this range some show the effects of much decomposition, while most are composed principally of dense, sub-lustrous trap.

The amygdaloidal trap and its relation to that which is non-vesicular.—Much amygdaloidal trap occurs in the East Haven Branford region, and while most of what is exposed is on the upper or concave side of Pond Rock, it is by no means confined to that position. The first and second members of the Pond ridge show a band of amygdaloidal trap six inches wide at the lower contact wherever this is exposed. The amygdules are of calcite. Amygdules of prehnite and calcite have been observed in the first range east of this. Prof. Davis says that no dikes are amygdaloidal in character,* defining a dike as trap crossing the strata of sandstone or shale more or less obliquely. In the second railroad cut east of Center street, Fair Haven East (G, on the map), there is a typical dike twenty-three feet wide. Four feet from the southeastern wall of this dike a band nine inches wide, containing amygdules of calcite and amethystine quartz extends in a line parallel with the wall of the dike. The band does not appear to be a vein. Section No. 19 is from this band of amygdaloid. The trap shows much more alteration than appears in Prof. Dana's section from the compact trap of the same dike. No augite is perceptible and much chlorite has been formed. A great deal of magnetite is present and most, if not all, of it is crystalline. The chlorite occurs disseminated through the rock and in the amygdules. The amygdules are various; some are all chlorite, others calcite with a border of chlorite, or vice versa; some are of quartz inclosing chlorite, or of calcite fringed with some zeolite, while others have quartz

* This Journal, III, xxiv, 346, 1882.

and calcite in the same cell. With the exception of this band of amygdaloid the rock of this dike is compact and almost lustrous. This dike is a member of the third range west of Pond Rock. The southern member of the first western range is remarkable for the number of amygdules and "pipestems" of calcite which it contains throughout its mass. A mile and a half north of the railroad there is a similar occurrence of amygdaloid. In both instances the trap is dull black in color without apparent crystalline structure. Three miles from the railroad the northern end of one of the members of this range appears as a dike intersecting the layers of sandstone (D, on the map).

The relation of the sandstone to the trap.—If the sandstone were oftener exposed in close proximity to the eastern slopes of the ridges of trap, there would have been less opportunity for discussion as to the relative age of the igneous rocks and the sandstone. The southern members of the third western range in and near the hill crowned by the New Haven Water Co.'s reservoir in the southern part of the borough of Fair Haven East are clearly seen to cut obliquely across the layers of sandstone. The walls of these dikes are very regular, much more so than those of the dikes in Wallingford and some other parts of Connecticut. The southern end of the member of this range next north of Hemingway mountain is exposed within 200 feet of the end of that ridge. The exposure is in the face of a bluff on the northern side of the road from New Haven to North Branford, and shows an irregular mass of trap lying between strata of sandstone. The latter is hard, mostly compact, and bright red. Heat from the trap, probably through the aid of moisture, has penetrated the joints of the overlying rock, vesiculating the sandstone. Tongues of trap also have pushed their way into crevices of the overlying sandstone. The dip of the sandstone is eastward and high, but was not satisfactorily determined.

The ridges of the second western range cut somewhat obliquely across the strike of the sandstone, but turn to the north a mile and a half north of the railroad just before they would otherwise have joined the first western range. No contact, or sandstone near a contact, is visible except at one place, where one small mass of trap shows itself in the road a third of a mile west of Lyman Granniss's house (C, on the map), as a distinct dike. Sandstone covers the top of the dike and shows that it has been pushed up by the trap. The sandstone beneath the trap of the first western range is exposed at many places on the western slopes of the ridges; but the overlying sandstone has been almost completely denuded from the ridges, its first occurrence being three miles from the railroad,

where it is in contact with the trap. At this place (D), which has been mentioned already, the strike and dip of the strata could not be satisfactorily determined, but it is evident that the trap intersects the layers obliquely and is a dike.

The underlying rock of Pond Rock is well exposed in the precipitous western faces of the first and second divisions, where the contact in many places is sixty feet above tide, and on the southern ends of the second and third. The sandstone is hard and compact at and near the contact, but farther down there are some loose and shaly layers. At the foot of the lake the strike of the sandstone is N. 10° E., its dip 50° E. A good cross-section of the contact is shown on the southern side of the railroad cut just east of the abutment of the wagon bridge over the cut; on the northern side the abutment hides the contact. At and near the contact the sandstone is much indurated, and the trap and sandstone are so firmly united that hand specimens of the contact can be trimmed with the hammer without loosening the joint. The trap seems to lie conformably upon the sandstone, which strikes N. 16° E., and dips 40° E. Most of the sandstone exposed in the western part of this cut is shaly and irregular in texture, but there are a few layers of compact rock. North of the railroad the underlying rock has been observed at only three places; everywhere else it is covered with the talus of the trap bluff or with earth, and no contact has yet been found. The first exposure is in the silver-seekers' trench, a mile and a half from the railroad, and is of fine soft shale and hard drab sandstone, the latter being the nearer to the trap. The next exposure is nearly two miles from this, in a ravine, on the northern slope of the rock, five-eighths of a mile east of the crossing of the Branford-Foxon road (R). The rock is a rather soft red shale some distance below the trap, with its strike S. 87° E., and its dip 25° S. Three-eighths of a mile farther east the third and most extensive exposure occurs. Near the probable location of the contact a rather soft, fine-grained, well laminated, brownish red shale is exposed. Its strike is S. 70° E., and its dip 23° S. Going down the ravine the edges of the strata are crossed, which are of coarse, hard red and brownish red sandstone interlaminated with shale which varies much in texture. The exposure extends for some 300 feet down the gentle slope.

The rock beneath the southern hook of Toket mountain is exposed at several places on the New Haven-North Branford road as a loose sandy shale with strike S. 65°–70° E., and dip about 18° N. This should be noted in connection with the strike and dip of the shale on the other side of the transverse valley beneath the northern hook of Pond Rock. No exposure near the contact was found.

Though many bowlders lie scattered over the eastern slope of the first member of the Pond ridge, none of the overlying sandstone has been observed *in situ* near the trap, except at the foot of the lake where a soft reddish brown micaceous shale forms the bank for some distance east of the trap. The contact of the two rocks, however, is not exposed. The shale dips steeply to the south of east. Prof. Davis, in speaking of the railroad through the ridge, says: "No overlying sandstone could be found."* It is there, however The exposure begins 135 feet east of the south abutment of the wagon bridge, and extends for about 150 feet along the track and southeastward for 500 feet to a point south of the ice-houses on the bank of the lake. Soil and underbrush prevent a determination of the exact extent of the occurrence up the slope, and grass hides the outcrop by the railroad. The rock is soft, sandy shale like that at the foot of the lake; for the most part dark red or reddish brown in color but dark green near the contact with the trap. The strike and dip of the shale were obtained at three places by digging and were: strike, N. 16° E., dip, 75° E. The dip of the eastern face of the trap at the contact is 80° or more toward the *north* of east. Fig. 3 is a plan showing the relation of the sandstone and trap at the two places just described, as well as the mode of union between the first and second, and second and third parts of Pond Rock. Fig. 4 represents the section exposed by the railroad.

North of the railroad the overlying sandstone is exposed at but two places near the trap of this ridge: these are on the southern side of the eastern prolongation of the rock. Half a mile east of the Branford-Foxon road a deep ravine (P) cuts into the southern slope of the trap and extends nearly to the top of the ridge. Into this ravine from the south a steep promontory of sandstone forty feet high projects, forming a triple ravine. The rock of this projection is a rather hard, very coarse sandstone containing some shaly layers; its strike is N. 60° E., and its dip 45° eastward. The contact is not exposed, but the sandstone is seen within fifteen feet of the trap. Half a mile farther eastward and well up on the slope is the last exposure of the overlying rock (H, on the map), and the contact is shown. The exposure is in a shallow ravine worn by a small stream. The trap is of dark reddish brown color, and, though filled with amygdules of calcite, is very firm where decomposition has not set in The sandstone is coarse and is very hard especially at the contact where it is firmly joined to the trap. Twenty-five feet down the hill from the contact and a few feet above the trap, the strike of the sandstone is S. 70° E., and its dip 42° S.

* Bull. Mus. Comp. Zool., 1883, p. 268.

South of the north end of Pond Rock and east of the Branford-Foxon road, between the main ridge and the first eastern range, much coarse, hard, yellowish-brown to red sandstone occurs together with some layers of shaly and soft stone. Overlying this and at least a third of a mile from the rock, we come upon a peculiar conglomerate containing bowlders and fragments of trap, which will be described later. It is briefly noticed by Percival,* who does not seem to have attached much significance to it. I have not found any bowlders or fragments of trap in the sandstone immediately overlying the trap of the main ridge or in the area just referred to between the coarse conglomerate and the main ridge.

Sandstone and sandy shale are exposed at several places, and to a considerable extent on the east side of Lake Saltonstall. For a third of a mile, beginning just east of the lake, the railroad cuts through two low hills which are composed of shale with an occasional stratum of sandstone. There is some variation of strike and dip in these cuts, but most of the strata of the western cut strike N. 10° E, while in the eastern the general strike is N. 20° E. The dip decreases from 61° E. near the lake, to 42° E. in the eastern part of the eastern cut. This shale is the continuation of that at the northern end of the second member of the main range which dips 75° E. Half way up the lake the eastern bank for several hundred yards is formed by a low bluff of sandy shale having its strike N. 52° E. and its dip 30° E. The lake is a fourth of a mile wide at this point. Within a quarter of a mile of the head of the lake coarse, rather hard sandstone is exposed at the water's edge under overhanging trees. The strike of the rock is N. 12° E., its dip 27° E. The trap is an eighth of a mile distant. Reference to the map will show that the strikes of these exposures of sandstone are nearly parallel to the trend of the heavy trap ridge across the lake from them.

Overlying sandstone is exposed at but few places near the first eastern range, and actual contact is shown at but one place: it is where the Branford-Foxon road crosses the ridge (near I, on the map). At this place a thin layer of very hard sandstone is firmly welded to the trap, which is vesicular. The strike of the strata is generally parallel with the trend of the range and the dip is high and away from the trap.

One contact of the trap of the second eastern range with the sandstone on its eastern side has been observed; this is at and near where the road from Branford village to "Cherry Hill" crosses the ridge. The locality is marked B on the map. Fig. 5 was made from a photograph of the contact exposed by the west side of the road. The strike of the sandstone is

* Geol. Conn., p. 324.

N. 45° E., its dip 60° E. Thirty feet southwest of this point the trap lies upon the edges of the upturned strata of sandstone, forming the contact-breccia from which thin section No. 15 was made. In the old quarry east of the road the eastern sandstone has a *westward* dip. The line of contact showing the gradual overthrow of the dip may be traced from the road into the quarry. North-northeast of this old quarry and nearly half a mile distant on another road much coarse, hard, dark-red sandstone is exposed, having its strike N. 45° E. and its dip 40° E. This exposure is just west of the southern end of the last member of the second eastern range of trap ridges.

Contact phenomena.—Contact with the underlying sandstone. The thin sections already mentioned as from contacts between the trap and the sandstone, when examined under the microscope, show no peculiar character that can surely be assigned to the heated state of the trap at the time of its rising. In No. 21*a*, which is from the contact with the underlying sandstone in the railroad cut through Pond Rock, the line of contact is strongly marked by an almost black band of iron-stained trap. The trap and sandstone seem to be cemented by the Fe_2O_3 rather than by calcite, which appears as amygdules in the trap.—*Contact with the overlying sandstone.* No. 18, from the southern slope of the northern hook of the same ridge, shows this contact. The trap is rather uniformly stained with Fe_2O_3, but there is no band characteristic of the contact, as in the slide just described. In the thin section some small inclusions of trap appear in the sandstone near the contact, but they are irregular in outline and show no signs of having been at all water-worn before they were surrounded by the sandstone. On the other hand particles of quartz, orthoclase and mica are isolated in the trap near the contact. No. 15, from the contact-breccia already described as occurring on the east side of the second eastern trap range, shows small angular masses of trap which appear to have been rolled up in a soft sandy shale and consolidated by Fe_2O_3. There are isolated inclusions of the shale in the trap. Hand specimens from the same locality show fragments of sandstone intimately mixed with the fragments of trap. This slide and No. 18 show a peculiar arrangement of the feldspar crystals in feathery aggregates. These tufts are most noticeable at the contact, but they are not confined to this position, and the ordinary lath-shaped crystals alternate with these aggregates at the contact, while some of the fragments in No. 15 do not show the structure at all. The tufts show only aggregate polarization. That they are composed of altered feldspar seems to be true from the fact that they are seen occasionally to grade off into the common lath-shaped crystals of plagioclase. They do not

seem to be sphærocrystals or the result of rapid cooling. No glass has been observed in these specimens, but the trap appears to be holocrystalline, except as alteration has taken place.

The macroscopical contact phenomena are well known, and have been described many times. They are, the induration or "baking" of the sandstone, the change of its color, and the production of chlorite and zeolites in its seams and cavities by the action of steam; while, in a thick sheet, the trap from the middle is much more coarsely crystalline than that from the sides. Prof. Davis states that the indurating or metamorphic effect of true dikes upon adjacent rocks is slight, and may be taken as extending from the dike to a distance equal to one-tenth of its thickness.* My own observations lead to the conclusion that no rule even approximately correct can be laid down. The most accessible of the pronounced dikes of the third western range are on the road from New Haven to East Haven half a mile east of Tomlinson's drawbridge (at F, on the map), and in the second and third railroad cuts east of Center street, Fair Haven East. The walls of some of these dikes show great variation in the amount of baking done to the different strata composing them. A layer of rock which has been compacted and indurated to a distance of from one to six feet from the trap will be overlain by shaly sandstone or coarse sand which has been affected in this manner but a few inches from the contact, and this in turn will be overlain by indurated rock. A nine foot dike on the East Haven road stands between walls of hard sandstone about 18 inches thick, but the same dike is exposed under a barn 450 to the north with its western wall composed of purplish black sandstone which has been indurated to a high degree for more than eight feet from the trap. The eastern wall has been removed, either naturally or artificially. This variation in the relative amount of baking of the different layers through which a dike has passed may be explained by supposing that some strata contained more water than others which were comparatively dry at the time of the eruption of the trap. Wet sand or other rock material being a better conductor than dry, the heat of the dike would penetrate farther into the wet layers than into the dry ones; furthermore the hot water and steam thus produced would dissolve silica, which, on being deposited again, would cement the particles of sand together. Dry heat not greater than that which the trap probably had at the time of its eruption would have no consolidating effect upon sand composed, as the Connecticut Triassic is, of quartz, orthoclase (and microcline) and mica.

* This Journal, III, xxiv, 346, 1882.

Sandstone containing trap.—This rock may be separated into two classes: (*a*) the contact breccia, which is the result of the rubbing of the trap against the sandstone on its upward or onward passage, (*b*) trap conglomerate, which is, probably, the effect of the action of aqueous forces. The contact breccia occurs on the east side of the range E. 2, (B), on the southern slope of the northern hook of the main ridge (H), and on the northern slope of the southern hook of Toket Mt. The trap-and-sandstone rock lying upon parts of the range E. 2, may possibly belong to this class. The trap conglomerate lies on both sides of the northeastern extension of E. 2, and east of its end, and, as has already been stated, the northern limit of the area is about a third of a mile from Pond Rock where nearest to it.

The region containing the most interesting part of the conglomerate, i. e. the ridges, is about three-fourths of a mile long from S.W. to N.E. and perhaps half a mile wide. There are several of these ridges each of which is from 300 to 400 yards long; and their general trend is N. 25° E. They are narrow, begin and end abruptly, have almost precipitous sides, and rise about 125 feet (aneroid measurement) above the brook and meadow bounding their region on the east and south. The valleys between them are narrow and from 40 to 60 feet deep. The outlines of the bowlder ridges are indicated on the map by dotted lines.

The rock of the ridges is exceedingly coarse. Well rounded bowlders a foot in diameter are very numerous, while others two, three and even four feet long are by no means rare. Much trap is present in the rock. In some of the western ridges the conglomerate is mainly composed of trap fragments with a small amount of coarse sandstone cement; the middle ridges contain many bowlders of quartzite, mica- and hornblende-schist, gneiss and granite; while the eastern ridges seem to have rather more of the latter constituents than of trap. Bowlders of coarse sandstone also occur in this rock, and many of the fragments of trap have coarse sandstone adhering firmly to them and forming a part of the bowlders imbedded in the conglomerate. The pieces of trap are either angular, subangular, or well rounded, compact or amygdaloidal, and quite fresh or much decomposed. Many of them contain long, vermiform cavities, either empty or filled with calcite, like the trap of the southern end of the ridge next west of Pond Rock. But few trap bowlders and those widely scattered occur in the western and southern part of the conglomerate area.

Main theories as to the origin of the trap.—Prof. Davis, in connection with his bibliography of the subject,* has given

* Bull. Mus. Comp. Zool., vii, 1883.

an abstract of the views of most of the writers who have dealt with the Triasssic trap; those, therefore, who desire to investigate in detail the history of the discussion are referred to his article. In 1833, Pres. E. Hitchcock supposed that the trap ridges were overflow sheets contemporaneous with the deposition of the sandstone;* but afterwards he modified his views so far as to hold that some of the sheets were intrusions.† H. D. Rogers claimed that the trap of the New Jersey Triassic region was erupted "through a series of nearly parallel fissures in the strata, and after their consolidation and subsequent disturbance."‡ Prof. J. D. Dana adopts this theory to account for the trap of the Connecticut valley Triassic.§ Most writers on the subject agree to the latter view, but Prof. Davis sustains the former, taking the ground that, while some of the ridges are intrusions, most of them are overflows, and that all were ejected before the tilting of the strata took place;‖ and further, that the present tilted position of the trap is due to monoclinal faulting of a sandstone-and-trap formation.

In the New Haven region the West Rock range, Pine and Mill Rocks, and the East Rock series of ridges are regarded by Prof. Davis as intrusive sheets and dikes.¶ The trap ridges in the eastern part of the town of New Haven, already described as the southern members of the third western range, would be recognized as dikes by any observer. In Connecticut it is only with regard to the great ridges which lie in the middle and eastern part of the Triassic area, and extend northward into Massachusetts that there is doubt as to the intrusive or extrusive origin of the trap. Pond Rock is one of these ridges; and this ridge, together, with its associated ranges, has been taken by Prof. Davis to be typical of the whole Connecticut system.** He says that Pond Rock is an overflow sheet on account of "its small metamorphic effect at the base, its decided amygdaloidal texture on its back or upper surface, its irregular and brecciated structure, and its alteration and hydration."†† The following facts bear on these arguments.

At the section exposed in the railroad cut the underlying sandstone was indurated for four or five feet from the trap. The thickness of the trap sheet here is about 84 feet. The extent of the induration is not proportionally much different from what is shown at undoubted intrusions, but I have already

* Rep. on Geol., Min., etc., of Mass., 1833, p. 243.
† Final Rep. on Geol. etc, of Mass., 1841, p. 653, but cf. p. 526.
‡ Rep. on the Geology of New Jersey, 1836, p. 160.
§ Manual of Geology, 1880, p. 423.
‖ This Journal, III, xxxii, 344, 1886.
¶ Seventh Ann. Rep't U. S. G. S., p. 463.
** Ibid. p. 346. See also 7th Ann. Rep. U. S. G. S., p. 478, where special stress is laid upon Pond Rock.
†† Bull. Mus Comp. Zool., vii, 269. Cf. this Journal, III, xxxii, 347, 1886.

shown that the absence of extensive baking of the contiguous strata would not alone prove much regarding the heat of the trap. Furthermore, as heat is propagated more readily in the direction of the strata than perpendicularly to them, the consolidating effect of a mass of trap lying conformably upon the sandstone would not be apparent as far from the contact as it would from trap intersecting the strata.

The amygdaloidal or, rather, the vesicular structure of eruptive rocks is supposed to have been produced wherever diminution of pressure allowed the highly heated water contained in their mass to expand into steam. The most favorable place for the production of vesicular lava is the upper part of a subaerial flow; but that the formation of vesicular lava, or trap, has not been confined to subaerial or subaqueous flows has been proven by the amygaloidal character of some of the dikes in the first and third ranges west of Pond Rock. The six inch band of amygdaloid at the botom of the main trap sheet can not be used as an argument either way, for it can be explained easily whether the trap be intrusive or extrusive.

The "irregular and brecciated" structure of the ridge is best shown at the junctions between the first and second and the second and third parts of it, and is not well developed anywhere else. Such a structure would be produced by the sudden cooling of liquid rock, and is frequently found near the walls of anhydrous as well as of hydrous dikes and intrusive sheets. Decomposition has been more extensive in the hydrous trap than in the other, which brings out the structure more plainly. A low dike about 130 feet wide is exposed on Farren Avenue, Fair Haven, the rock of which is as much broken and decomposed as that at the lake. This is distinctly a dike, the strata across which it cuts being exposed on each side of it. This "brecciated structure," therefore, being produced by local conditions, proves nothing with regard to the intrusive or extrusive character of a trap sheet.

The "alteration and hydration" of the Pond Rock trap is Prof. Davis's fourth argument for the extrusive, or overflow, character of the sheet. The alteration of the rock of this ridge varies greatly in degree, in different places. At the foot of the lake and at the railroad cut, decomposition has gone so far as even to destroy the crystalline texture of the rock, but the trap of most of the ridge is dense, tough and sublustrous and shows its crystalline structure plainly, though much chlorite is present. Slide No. 23 is from the middle of the sheet on the northern hook of the ridge. The Farren Avenue dike is thicker than the Pond Rock sheet at either the wagon-road or the railway crossing and is much decomposed throughout its mass; and many of the dikes and intrusive sheets

which form the range next west of Pond Rock show as much hydration as any part of the great ridge itself, so far as macroscopical examination goes. The lavas of Kilauea show no hydration of their constituents, and other modern lavas are like them in this respect. Daubrée has shown experimentally that liquid rock under great pressure will absorb into its mass vapors from surrounding material, and hence that the vapors formed by liquid rock ascending through wet porous rocks may find readier means of egress with the moving mass than back through the interstices of the porous rock against hydrostatic pressure. The present hydrated or non-hydrated character of the trap, therefore, is not dependent upon its being an overflow sheet: it may be due to the nature of the strata through which it passed.

Better than all this negative evidence as to the character of Pond Rock is the positive testimony of the overlying sandstone and shale. Wherever these are exposed near or against the trap their dip is much higher than that of the rock underneath the sheet (vid. pp. 28, 29 supra); the difference of dip between the underlying and the overlying strata in the railroad cut being 35°, and on the northern hook of the ridge 17°. The *overlying* sandstone on the northern hook is hard baked, and at the contact is mixed with the trap in such a way as to show that it must have been laid down before the advent of the trap (vid slide No. 18), and also that the difference of dip cannot be ascribed entirely to lateral shoving since the eruption of the trap. These facts appear to sustain fully the conclusion that Pond Rock is an intrusive sheet.

The breaks in the southern part of the ridge, near the railroad cut and at the foot of the lake, seem to be due to irregularities in the original fissure rather than to faulting after the trap was in place.* The thickness of the trap sheet diminishes greatly and rapidly at each place, leaving a narrow isthmus to connect the larger portions. The sheet thins from 225 feet, a short distance north of the railroad, to 84 feet in the cut and thickens again to 250 feet or more in the second part of the ridge; while at the foot of the lake the trap is apparently less than 40 feet in thickness. The isthmus at the cut is very short, but from the end of the lake the narrow portion extends more than 200 feet southward. If these were faults we should expect to find approximately the same thickness of the trap sheet on both sides of the fault-plane, unless the trap on one side had suffered more from erosion than the other. That the narrowing in Pond Rock is due to the original fissure and not to erosion, glacial or other, is proven by the presence of

* Prof. Davis says that these notches are "oblique faults" in the trap. Vid this Journal, III, xxxii, 346, 1882.

the overlying shale on the thin part. Again, there should be close similarity between the rocks on each side of such small dislocations as these are, if they were faults; but the trap rock in the necks is much more fractured in structure, than it is either just north or south of them, which is a result of its being thinner at the time of eruption and therefore more rapidly cooled.

The crescentic form of many of the outcrops of the trap has called out much speculation as to its origin. The best examples of these "crescents" are Pond Rock and Toket Mt. Prof. Davis advances the idea that the form is due to "a combination of faults and faint dish-like folds,"* and would make the ranges east of Pond Rock confirm the theory, saying that the second range is but the eastward outcrop of the first, brought up by the saucer-like synclinal. The facts do not appear to me to support this conclusion. His scheme demands that the sandstone near the second eastern range should dip to the west, while that below and above the southern portions of the main sheet and the first eastern should dip northward. But the sandstone on both sides of the main sheet in the railroad cut dip 16° south of east; at the foot of the lake the direction of dip is about the same, and there is no reason to suppose that there is any change throughout the remainder of the ridge, which is short and nearly straight. No outcrop of the sandstone has been observed near the southern end of the first eastern range, but the shales in the railroad cuttings just east of the lake, and on the Branford turnpike very near the crystalline rocks, dip high to the south of east. At two places half a mile apart, near E 2, the sandstone is exposed with its strike N. 45 E., and its dip at one place 60° and at the other 40° toward the southeast, while the only westward dip observed in connection with this range is in the old quarry east of the road from Branford to "Cherry Hill," and this is probably due to an overthrow by the trap. Fig. 1 was prepared from data obtained in the field. Near the northern part of the main sheet and the first eastern range most of the sandstone is conformable with the trap.

Concerning Pond Rock and Toket Mt., which he considers parts of one great sheet, Prof. Davis says: "Toket Mountain is not separated from Pond Mountain by a fault, but by erosion on a transverse flat-arched anticlinal, clearly defined by the dip of the adjacent conformable sandstones."† A faintly developed transverse anticlinal is called in to explain a slight indentation in Toket Mt. north of its middle, and a large one

* This Journal, III, xxxii. 347, 1882. In the 7th Ann. Rep't U. S. G. S., just issued, this theory is more fully worked out.

† This Journal, III, xxxii, 347, 1882.

to account for the wide separation of the Mt. Holyoke and Deerfield ranges in Massachusetts, besides others of greater or less extent for other parts of the valley. The cause assigned for the production of these "transverse anticlinals," is that they would be a result of the general movement which caused the usual eastward dip of the sandstone, acting in connection with an uneven foundation of gneisses and schists. Horizontal compression produces folds the axes of which are at right angles or nearly so to the direction in which the force acts. Prof. Davis's theory, however, demands the frequent repetition on large and small scales of very peculiar conditions in the crystalline rocks, which as yet have no proof in observation and do not seem to be probable; and it seems to necessitate a homogeneity and a tenacity in the sandstone which that rock does not possess. Further evidence against his "faint, dish-like folds" is found in the broken character of the subordinate ranges, and in the high southeastward dip of the sandstone near the range E2 and the whole southern part of the Pond Rock region. The strike of the sandstone in the southern part of the Connecticut Triassic region, except in some places near the trap, may be taken as averaging N. 15° E., therefore the tilting force must have acted from S. 75° E. with considerable constancy: such a force could not have produced an anticlinal like that between Pond Rock and Toket Mt. The underlying shale on the northern hook of the former has its strike S. 69° E., and its dip 23° S., while the sandy shale three quarters of a mile away beneath the latter strikes S. 65°–70° E., and dips 18° N. There are some irregularities of strike and dip in the valley between the mountains but the axis of the arch may be taken as being W.N.W.–E.S.E., or nearly parallel to the direction of the general force tilting the strata of the Connecticut valley Triassic. Since the transverse anticlinal cannot owe its existence to the general tilting force, it may be assigned to another one, viz: dikes having a general W.N.W. course, most of which have not yet been exposed. A ridge which may be the southeastern of such a line of dikes is exposed northeast of the end of Pond Rock on the Branford–Northford road. The exposure is some 500 feet long on this road and the crossroad to North Branford village, and is about 100 feet wide. No sandstone is exposed near it to prove or disprove the theory. It may be of interest to note, in passing, that this little ridge is curved with convexity to the west. The range of trap ridges next west of Toket Mt. ends at the entrance to this transverse valley (K, on the map); if extended, it would join the trap ridge just described and satisfy the theory. The observations necessary to establish this theory for other localities have not been made.

H. D. Rogers proposed a theory to account for the crescentic form of the ridges in New Jersey, which will hold very well for those in Connecticut, especially if the theory just proposed to account for the dip of the underlying sandstone be correct. He said: "The sandstone being disrupted in a plane parallel to the dip, the beds on the upper side of the sloping dyke will be lifted off from those upon which they reposed, and in this tilting of the beds, there will arise towards the extremities of the fissure seams or transverse cracks extending in the direction of the dip."* This theory readily accounts for the conformability of the overlying sandstone in the "hooks," for the trap flowing into these transverse cracks would modify the dip of the upper sandstone so as to make its direction more or less nearly the same as the slope of the upper surface of the intrusive rock. That the fissures were sometimes originally curved is shown by curved dikes and lines of dikes in the region west of Pond Rock, while the curved outline of many of the ranges in the whole valley is produced by what Percival calls the "advancing or retreating order" of position of nearly straight fissures. It has been shown experimentally that when one set of fissures is produced in strata, another subordinate set transverse to them may be produced at the same time; this fact would explain the network of dikes east of Foxon in the northern part of East Haven.

Hemingway mountain is another of the trap ridges in the region which Professor Davis has claimed to be overflow sheets. Its intrusive character is shown at its northern end where the extremity of the trap sheet stands between walls of sandstone, both of which have been much indurated and otherwise affected by the heat of the trap; and is also indicated by the trap of the ridge next west, which is exposed almost horizontally between strata of sandstone, the upper of which it has penetrated and scorified as well as hardened. North and northwest of Hemingway mountain, within a mile, there are several high ridges which seem to be dikes; they begin and end abruptly and both sides are precipitous, while the outliers of some of them are seen squeezed in between layers of sandstone. The sandstone has been indurated to such an extent and degree that it stands in ridges as high as those of the trap with sides almost as steep.

The heavy trap conglomerate south of the north end of Pond Rock is a puzzling factor in the problem, not only on account of the trap in it and its relation to the trap ridges near which

* This Journal, xlv, 334, 1843. Report of meeting of the Association of American Geologists and Naturalists. At this meeting a committee, consisting of Professor Silliman, J. D. Whelpley and H. D. Rogers, was appointed to determine, if possible, the correctness of this theory. I have not found any report of the results of their investigations.

it occurs, but also on account of the great size of the bowlders composing it. In the southern part of the Connecticut valley this heavy conglomerate occurs only in connection with the ranges of trap southeast of Toket Mt. and those east of Pond Rock, all of which are very near the eastern crystalline rocks. The most probable sources of the trap in the conglomerate are the heavy sheets near by, though the bowlders do not much resemble the trap of the great ridges. But the conglomerate does not affect the question of the intrusive or extrusive origin of Pond Rock and Toket, for in each case much comparatively fine sandstone containing no trap except such as forms a contact breccia lies between it and the main ridge. The formation is older than the ranges east of Pond and Toket; for the trap of the range east of the latter lies on top of it, and that of the ranges east of the former intersects it.

The age of the trap.—Professor Davis concludes that all the trap was ejected before the tilting of the sandstone took place. This does not appear to be true in the region I have examined. The sandstone of the western part of the region was much faulted and broken in the process of upheaval; but the true dikes do not appear to have been faulted, and therefore must have been formed since the tilting of the strata. The fissures through which the trap came up may have been made in connection with the tilting. Pond Rock is an intrusive sheet, but the evidence regarding its age seems to be incomplete. It is certainly more recent than the youngest strata now exposed in this part of the valley (except the trap conglomerate), for it is intruded between them. The crescentic form, if we accept Rogers's theory of its origin, might be taken as an indication of intrusion after the tilting. If Pond Rock be of more recent date than the upheaval of the sandstone, then the first eastern range is still more recent, for its northern members separate the strata of the conglomerate which was probably derived in part from Pond Rock. If we make Pond Rock intrusive after the tilting, we must suppose the heavy trap conglomerate to have been made later than the Triassic era; which may be a rather violent hypothesis, as unconformability between the conglomerate and the sandstone has not been proven. The second eastern range consists of dikes, as is shown by the dip of the adjacent sandstone and the position of the trap with reference to it; and, on the same evidence as is presented by the western dikes, these are more recent than the tilting of the sandstone.

Conclusions.—My observations, then, lead me to agree with Professor Davis in so far as to make some of the trap older than the very last of the sandstone formation. Probably Pond Rock is this older trap; and the southern part of the range

W. 1 also may antedate the tilting of the sandstone. I differ from him, however, in making all the trap of the East Haven-Branford region intrusive, and the western dikes, at least, of later origin than the tilting of the sandstone.

NOTE.—On the accompanying map the exposures of trap are represented by solid black areas, except that the trap of Pond and Toket Mts. is cross-lined. Separate ridges that are supposed to be connected near the surface of the earth are joined by a light line. Where sandstone has been observed between the ridges of trap, the fact has been indicated by thickly-set dots. Some strikes and dips of the sandstone have been recorded by using T's and placing the number of degrees from the horizontal near the stem and the declination from north or south near the cross line. The small circles in E. 2 represent the trap-and-sandstone rock there. In the figures trap is indicated by crossed lines; and sandstone and shale by parallel lines, the former by those which are the farther apart.

A—Silver-seeker's trench, west side of Pond Rock, sandstone.
B—Contact of trap of E. 2 with sandstone where the road from Branford to "Cherry Hill" crosses the ridge.
C—Lyman Granniss's house.
D—Exposure showing dike-like character of some members of W. 1.
F—Dikes on the road from New Haven to East Haven.
G—Dike in second railroad cut east of Center st., Fair Haven.
H—Exposure of overlying sandstone, northern hook of Pond Rock.
I—Branford-Foxon road.
K—Southern end of trap range next west of Toket Mt.
L—Southeastern end of range from Peter's Rock.
M—Southern end of ridge next north of Hemingway Mt. where it lies between layers of sandstone.
O—Southern most exposure of trap conglomerate.
P—Triple ravine formed by ridge of sandstone jutting into a ravine in the trap of Pond Rock.
Q—Complex set of dikes northeast of Foxon. No sandstone is exposed between these ridges and there probably is more or less connection between most of them. They seem to occur along two or three lines.
R—Exposure of sandstone near northwest angle of Pond Rock. Strike S. 87° E., dip 23° S.

Yale University, June, 1889.

ART. XLVIII.—*A Theory of the Mica Group;* by F. W. CLARKE.

EVER since its publication in 1878,* in spite of a few dissentient voices, Tschermak's theory of the mica group has been generally in vogue. Nevertheless, upon careful inspection, the theory seems open to serious objections. In brief, omitting details for the present, Tschermak regards the micas as made up of four fundamental molecules; to which the following formulæ are assigned:

1. $R'_6Al_8Si_6O_{24}$.
2. $Mg_{12}Si_6O_{24}$.
3. $H_8Si_{10}O_{24}$.
4. $F_{24}Si_{10}O_8$.

R′ stands obviously for K, Na, Li, or H; Mg is equivalent to Fe″, and aluminum may be replaced by ferric iron. The first of these formulæ represents fairly well the composition of normal muscovite and paragonite; the only objection to it being that, as used by Tschermak, it assumes the double atom of quadrivalent aluminum. Since it has been proved by Nilson and Petterson, Combes, Quincke, and others, that aluminum is truly trivalent, the formula should become $R'_3Al_3Si_3O_{12}$; which, as I have shown in several previous papers, may be regarded as a substitution derivative of normal aluminum orthosilicate.

The second, third, and fourth of Tschermak's formulæ represent hypothetical compounds; the second being a polymer of chrysolite, to which, in nature, some varieties of talc offer the nearest approximation. The third and fourth formulæ are analogous to no known substances, and are, moreover, improbable chemically. No mica, by itself, resembles in composition either of the three. One formula, then, corresponds to an observed mineral, while the others are purely speculative; a state of affairs which is not altogether satisfactory. Furthermore, the four formulæ represent as many distinct chemical types, whereas it would seem as if all the true micas should be reducible to one general form, under which their isomorphism would be more readily intelligible.

There is still another objection to Tschermak's system of formulæ, which, if sustained by future investigation, will be fatal. Under his theory the ratio of oxygen to silicon in the micas can never exceed four to one; while the published analyses of micas often show values far higher, ranging in

* Ber. Wien. Akad., lxxviii, 5. Zeitschr. Kryst., iii, 122.

some cases nearly up to five. In certain instances, as Tschermak himself suggests, this excess of oxygen over the orthosilicate ratio may be due to water enclosed between micaceous laminæ; and in other cases it may indicate partial alteration. There are micas, however, in which neither of these explanations appears to be satisfactory, and the weight of evidence goes to show that the excess of oxygen is essential. If this be true, Tschermak's theory is so far inadequate; for to such micas it cannot apply without serious modification. The question can only be settled experimentally, but at present the objection raised by it cannot be ignored. Some of its details will be considered later.

Now, in order to conclusively replace Tschermak's theory by something better, two fundamental conditions must be satisfied. First, all micas, with the possible exception of the pseudomica, margarite, should be reducible to one general type of formula, which shall express all known relations equally well with the formulæ proposed by Tschermak. Secondly, hypothetical compounds must be so far as possible avoided, and admitted into consideration only when their assumption can be shown to be absolutely necessary. The chief difficulties to be overcome are the variations in the silicon-oxygen ratio, and the presence of fluorine in many common micas.

In a number of papers published during the past four years,* I have sought to show that all orthosilicates containing aluminum may be represented as substitution derivatives of the normal salt $Al_4(SiO_4)_3$. Muscovite can be so derived, theoretically, through the replacement of one aluminum atom by R'_3; and in many other cases we have equal simplicity of expression. The ferro-magnesian micas, the phlogopites, and the lepidolites, however, are less simply derived; and I have in certain cases suggested a linking together of two orthosilicate nuclei to the group $Al_2(SiO_4)_3$ in order to account for their formation. I am now inclined to believe, however, that all the true micas are referable to the same general type, and have discussed nearly one hundred published analyses from that point of view. If we take magnesium as a generic representative of the bivalent metals, and give univalent elements or groups the general symbol R, we can imagine the following derivatives of $Al_4(SiO_4)_3$ as easily possible.

1.
$$\begin{matrix} & /SiO_4{\equiv}R_2 \\ Al-SiO_4{\equiv}Al \\ & \backslash SiO_4{\equiv}Al \end{matrix}$$

2.
$$\begin{matrix} & /SiO_4{\equiv}R_2 \\ Al-SiO_4{\equiv}R_2 \\ & \backslash SiO_4{\equiv}Al \end{matrix}$$

3.
$$\begin{matrix} & /SiO_4{\equiv}R_2 \\ Al-SiO_4{\equiv}R_2 \\ & \backslash SiO_4{\equiv}R_2 \end{matrix}$$

* See this Journal, Nov., 1886 and Aug., 1887. Also Amer. Chem. Journ., x, 120, March, 1888.

4.	5.	6.
$\diagup SiO_4 \equiv MgR$	$\diagup SiO_4 \equiv MgR$	$\diagup SiO_4 \equiv MgR$
$Al - SiO_4 \equiv Al$	$Al - SiO_4 \equiv MgR$	$Al - SiO_4 \equiv MgR$
$\diagdown SiO_4 \equiv Al$	$\diagdown SiO_4 \equiv Al$	$\diagdown SiO_4 \equiv MgR$

To these we may add, as No. 7, the compound $Al_2(SiO_4)_6Mg_9$, the bivalent analogue of No. 3, and identical in type with it. Now, so long as we have only orthosilicate micas to consider, these seven formulæ cover all their variations in composition; provided that fluorine, when present, is represented either by $-Mg-F$ or $-Al=F_2$, univalent groups which are included under the general symbol R′. Most of the micas appear as intermediate mixtures of these presumably isomorphous types. No. 1 represents muscovite and paragonite, and No. 6 agrees tolerably with some phlogopites. No. 2 may be resolved into a mixture, in equal molecules, of No. 1 and 3; and similarly No. 5 may be regarded as composed of Nos. 4 and 6. Nos. 5 and 6, moreover, may be simplified into mixtures between 3 and 7, so that numbers 1, 3, 4 and 7, represent all the necessary relations. Even No. 4 is possibly superfluous.

So much for the normal orthosilicate micas. But in the lepidolites, phlogopites, and some muscovites, the oxygen-silicon ratio is low; and in the lepidolites especially it approximates more or less closely to the metasilicate type. This order of variation is clearly established, while variations in the opposite direction, that is toward excess of oxygen may be questionable. If, however, in any mica the oxygen, can be properly in excess of SiO_4, that excess may be fairly regarded as present in the group $-Al=O$, which is obviously equivalent to $-Al=F_2$, and takes place with the latter as a part of R′. Examples of this kind are given in one of my former papers.* In all such cases the system of formulæ proposed above applies perfectly, and needs no qualification. The variations in R′ always fall within its limits.

The lower values for the silicon-oxygen ratio are explicable as follows: The polysilicic acid $H_4Si_3O_8$, which, like H_4SiO_4 is tetrabasic, is represented in nature by orthoclase and albite. In anorthite we have an orthosilicate, and its mixture with albite gives, as is well known to all mineralogists, the intermediate triclinic feldspars in which pseudo-metasilicate ratios often appear $H_4Si_3O_8 + H_4SiO_4 = H_8Si_4O_{12} = 4H_2SiO_3$. If we assume a similar state of affairs among the micas, and regard orthosilicates and polysilicates as isomorphously miscible, the lepidolites and other low-oxygen micas are completely accounted for. We have then the same system of general formulæ for all micas, the normal salts $Al_4(SiO_4)_3$ and $Al_4(Si_3O_8)_3$ being the theoretical starting points for derivation. In every

* This Journal, Aug. 1887, p. 131.

case the composition of a mica becomes reducible to the one general type, under the proposed theory of substitution. Representing the groups SiO_4 and Si_2O_6 by the common symbol X, the micas all fall within limits indicated by the formulæ $Al_3X_3R'_3$, and $AlX_3R'_9$. We may test this principle and the preceding formulæ by application to actual examples, taking the different micas group by group. In general, however, I must omit the details of the individual analyses discussed, as I hope to publish them more fully hereafter.

Muscovite.

This mica, the most typical and most abundant of all, is also the simplest chemically. It is best represented by formula No. 1, which, in its special application becomes ordinarily $Al_3(SiO_4)_3KH_2$, with some variation in the ratio of K to H. In most cases muscovite contains small amounts of magnesia and ferrous iron; and if these are deducted, as shown in formula No. 6, the residue agrees still better with formula No. 1. Fluorine is often present in small quantities, and appears to vary in relation to hydrogen, being lowest when the latter is high, and the reverse. Hence it is probable that the group AlF_2 replaces H rather than K. This is shown more clearly among the lepidolites, in which fluorine reaches a maximum, while the proportion of water is almost insignificant.

Some muscovites, however, vary from the normal compound in that they contain more silicon and less oxygen; thus approaching somewhat to lepidolite. These micas, which Tschermak has called "phengites," are represented by him as mixtures of $Al_6R'_6Si_6O_{24}$ with $H_8Si_{10}O_{24}$ in the ratio of three to one. It is simpler, however, to follow out the analogy offered by the feldspar group, and to assume the existence in muscovite of the isomorphous compound $Al_3(Si_3O_8)_3KH_2$. True, this compound has not been found by itself in nature, and so far its assumption is objectionable. But the compound $H_8Si_{10}O_{24}$ is also non-existent, is different in type from ordinary muscovite, and is not easily conceivable as a definite entity. The alternative which I offer for it is therefore, it seems to me, more philosophical and more satisfactory; and it accounts completely for all the oxygen variations in muscovite. For the sake of brevity, however, we may well retain the name of *phengite* in our vocabulary, and may speak of micas containing the Si_2O_6 groups as more or less *phengitic.*

Lepidolite.

In this species, the most phengitic of all the micas, we find little water, high fluorine, and a very notable proportion of lithia. It always occurs with muscovite, and commonly im-

planted upon the latter, in such a way as to clearly indicate its later formation. In composition it is regarded by Tschermak as a mixture of $Al_2K_2Si_4O_{24}$ with $F_{24}Si_{10}O_8$, the former being about half replaced by the corresponding $Al_2Li_2Si_4O_{24}$, and the latter in part by $H_8Si_{10}O_{24}$. The objections to this interpretation have already been pointed out.

Under the new mode of interpretation, lepidolite becomes much simpler. In every case, if we eliminate traces of magnesia and iron, as was done under muscovite, the residue corresponds sharply to a mixture of the two molecules $AlX_2R'_2$ with $Al_2X_3R'_3$, X representing Si_3O_8 and SiO_4 indiscriminately. In the purest lepidolites these molecules are in the ratio 1:1, corresponding to $Al_2X_3R'_5$; but in general the second molecule is slightly in excess, due to small admixtures of normal muscovite. In the discussion of fourteen published analyses of lepidolite the ratio $Si_3O_8 : SiO_4$ varies from 1:1 to 1:3; and the sum of $Li + AlF_2$ appears to be directly related to the proportion of Si_3O_8. In brief, expanding the general formulæ given above, typical lepidolite may be expressed by the two compounds $Al_2(SiO_4)_2KHLi$ and $Al(Si_3O_8)_2K_2Li_2(AlF_2)_2$ in equal molecules. All the variations may be accounted for by admixtures of muscovite.

Two other highly fluoriferous lithia micas, cryophyllite and zinnwaldite, may properly be considered here. For the former we have Riggs's analysis, and for the latter, analyses by Berwerth and Rammelsberg. From these analyses we get the following empirical formulæ: 1, Riggs, cryophyllite; 2 and 3, Berwerth and Rammelsberg, zinnwaldite.

1. $Al_{186}Fe''_{94}K_{256}Li_{224}H_{146}(AlF_2)_{178}(Si_3O_8)_{227}(SiO_4)_{168}$.
2. $Al_{239}Fe''_{156}K_{236}Li_{216}H_{102}(AlF_2)_{209}(Si_3O_8)_{151}(SiO_4)_{312}$.
3. $Al_{244}Fe''_{167}K_{242}Li_{224}H_{116}(AlF_2)_{200}(Si_3O_8)_{156}(SiO_4)_{306}$.

Condensing these formulæ they become:

1. $Al_{186}Fe_{94}R'_{204}X_{412}$.
2. $Al_{239}Fe_{156}R'_{765}X_{463}$.
3. $Al_{244}Fe_{167}R'_{782}X_{462}$.

Expanding again, these formulæ give:

1. $31(AlX_3Fe''_3R'_3) + 81(AlX_3R'_9) + 25(Al_2X_3R'_3)$.
2. 62 " +49 " +43 "
3. 56 " +53 " +45 "

The component molecules here correspond essentially to Nos. 6, 3, and 1 of the fundamental series, the last one in each expression being muscovite. The method of factoring here shown I have adopted throughout my investigation of the micas, and I have found very few trustworthy analyses to

which it does not apply. Even the exceptions are apparent only, and can be met by a modification of the process which is somewhat more generalized. Whether the separate components of R′ and of X can be distributed with certainty among the several molecules is an outstanding question, but not, I believe, an unanswerable one.

Phlogopite.

In order to ascertain the composition of this mica I have discussed the four analyses cited by Tschermak, (*l. c.*), and three of Rammelsberg's.* All of these are fluoriferous, and all but one are phengitic, the exception in the latter case being the Edwards, N. Y., mineral, which Tschermak regards as somewhat altered. I have also considered the new phlogopite from Edwards, recently described by Penfield,† which is remarkable as being free from fluorine.

In general, phlogopite agrees quite closely with the formula $AlX_3Mg_3R'_3$, but there are often variations which are not easy to explain, and which are greater than can be accounted for by presumable errors of analysis. They may be due in part to impurities arising from the great alterability of the mineral; but I am inclined to trace the difficulties to other sources.

In order to satisfy the conditions of the formulæ proposed by me, it is necessary that the aluminum atoms, apart from AlF_3, shall be in number at least one-third of X. But in several phlogopites the alumina is too low to fulfil this requirement; and the difficulty is best obviated by assuming an MgF group in place of AlF_3. In Penfield's phlogopite, however, there is no fluorine, and yet the alumina is considerably—about two per cent—too low. If we suppose in his mineral, which came from a talc mine, a small admixture of talc, the residue agrees with the theoretical formula, X being wholly SiO_4. But if that impurity is absent we must assume that the mica differs from ordinary phlogopite by containing the group $-Mg-OH$ instead of the usual MgF. The composition of the mineral then reduces to the uniform type. I am now disposed to believe that phlogopite differs from the other micas in that it contains these special groups MgF and MgOH, both as part of R′; but the supposition is not yet fully justified. It is, however, I think, susceptible of experimental investigation, and a laboratory research upon the problem is now being carried out under my direction. Apart from that, the supposition is strengthened by the composition of certain vermiculites; some of which have certainly been formed by the alteration of phlogopite. Three of these interesting minerals are

* Wied. Annal., ix, 129. † This Journal, III, xxxvi, 329.

represented fairly well by the subjoined formulæ, in which ferric iron, present in small quantities, has been reckoned with the alumina.

$$\begin{array}{l} \diagup SiO_4 \equiv (MgOH)_3 \\ Al—SiO_4 \equiv Al \\ \diagdown SiO_4 \equiv Al \\ +3H_2O. \end{array} \qquad \begin{array}{l} \diagup SiO_4 \equiv \\ Al—SiO_4 \equiv \\ \diagdown SiO_4 \equiv Al \\ +3H_2O. \end{array} \left\{ Mg_3 \right. \qquad \begin{array}{l} \diagup SiO_4 \equiv MgH \\ Al—SiO_4 \equiv MgH \\ \diagdown SiO_4 \equiv Al \\ +3H_2O. \end{array}$$

Dudleyite. Culsageeite. Jefferisite.

It is by no means certain that the vermiculites are so simple in composition or so definite as these symbols would seem to indicate; but the formulæ are decidedly suggestive, and they show how clearly the relations between the micas and their derivatives may be expressed.

The ferro-magnesian micas.

This group of micas, which includes biotite, lepidomelane, annite, haughtonite, siderophyllite, and other supposed species, is apparently quite complex. In place of magnesia, ferrous iron is often predominant; in the lepidomelanes, ferric iron replaces aluminum, and in forty-four out of the fifty-six analyses discussed, the oxygen was in excess of SiO_4. Only seven of these micas appeared to be phengitic; and only sixteen of the analyses reported fluorine in small quantities. In six instances AlO and AlF_3 were both absent.

For thirty-four of the micas in this group, formulæ could be easily computed upon the lines already followed. That is, in each case the composition was represented by a mixture of $AlX_3R''_3R'_3$ with $AlX_3R'_9$ and $Al_3X_3R'_9$, the second or third of these molecules being occasionally absent. Among the lepidomelanes there was an approximation to a distinctively muscovitic type; and in two cases formula No. 4, $Al_3X_3R''R$, seemed to apply. With each mica an attempt was made to determine the proportions of the several admixed molecules; but the results, although numerically conformable to the general theory, were not absolutely conclusive. The chief difficulty lay in the uncertainty attaching to the water determinations, upon which the question of oxygen excesses depends. Since water has a low molecular weight, a small error in its estimation becomes relatively large in the molecular ratios; and two sorts of errors are presumable. First, an excess of water may be enclosed mechanically in the material analyzed; and secondly, a determination by simple ignition is likely to be too low because of the oxidation of ferrous iron. If these errors occur together, they obviously tend to compensate each other; but either one alone seriously affects the coefficient of R′, and appears in the ratio between $Al_3X_3R'_9$ and $AlX_3R'_9$.

In twenty-two of the fifty-six analyses, however, the symbol $AlX_3R''_3R'_3$ failed to account for all the bivalent metals, iron and magnesium. In these cases R″ was in excess of R′, and it became necessary to make use of formula No. 7, $Al_2(SiO_4)_6R''_9$. With the aid of this expression, all the ferro-magnesian micas without a positive exception, including the phlogopites, were resolvable into mixtures of $Al_2X_6R'_3$, $AlX_3R'_3$, and $Al_2X_6R''_9$. Upon this basis formula No. 6 becomes useless, for $AlX_3Mg_3R'_3$ is evidently equivalent to a mixture, in equal molecules, of $AlX_3R'_3$ and $Al_2X_6Mg_9$. All micas, then, so far as the analyses are authentic, may be represented as mixtures of the molecules 1, 3, and 7, these being symbols of one and the same general type. The magnesium (or ferrous) salt thus assumed, moreover, is not absolutely hypothetical; for, with six molecules of water of crystallization added it approximates to certain individuals of the chlorite group. Some examples of penninite, for instance, approach rather closely to the composition $Al_2(SiO_4)_6Mg_9 . 6H_2O$, which requires 38·71 of silica, 10·97 alumina, 38·71 magnesia, and 11·61 water. We have thus a clue to the constitution of the chlorites, by means of which they may be brought into simple relations with the micas. This problem is now under investigation, and I can only state as a probability that most of the chlorites, if not all, may be represented as mixtures of three fundamental molecules, $Al_2X_6R''_9$, $6H_2O$; $AlX_3R''_3H_3$, $3H_2O$; and $Al_2X_6(MgOH)_6$. I hope to speak more positively upon this subject before long; at present I need only point out that two of these molecules are simple hydrates of Nos. 6 and 7, while the third is covered by the general symbol No. 3, in which R'_6 becomes $(MgOH)_6$. The easy alterability of garnet, $Al_2(SiO_4)_3R''_3$ into mica and chlorite, is a suggestive bit of evidence bearing upon these expressions.

Margarite.

Whether this species is to be considered as a true mica or not, is perhaps an open question. Its composition is relatively simple, and is represented by the empirical formula $H_2CaAl_4Si_2O_{12}$. Structurally, this may be written in two distinct ways, as follows:

$$\begin{matrix} \diagup OH \\ Al—SiO_4{\equiv}CaH \\ \diagdown SiO_4{\equiv}(AlO)_3 \end{matrix} \qquad \text{or} \qquad \begin{matrix} & SiO_4{\equiv}(AlO)_3H. \\ Ca\diagup & \\ & \diagdown SiO_4{\equiv}(AlO)_3H. \end{matrix}$$

The first of these symbols derives the compound from the normal aluminum salt, and indicates a similarity of type with the micas proper. The second assumes a normal calcium salt as the point of derivation; and I am unable as yet to assign a

distinct preference to either expression. A careful study of the genesis and associations of margarite may determine which of the two formulæ is the better. Possibly the occurrence of the mineral in such mixtures as euphyllite, and the probable existence of a similar barium salt in œllacherite points to the first of the two formulæ as the better. The common occurrence of margarite with diaspore also points in the same direction.

The Clintonite group.

These minerals, the so-called "brittle micas," have also been discussed by Tschermak,* who includes under this heading seybertite, brandisite, xanthophyllite, chloritoid, masonite, ottrelite, sismondine, and sapphirine. Physically, they are closely related to the micas proper, and to margarite in particular; but chemically they are much more basic. The first three species Tschermak regards as mixtures of the hypothetical compounds $H_2CaMg_2Si_2O_{12}$ and $H_2CaMgAl_2O_{12}$; to chloritoid he assigns the composition $H_2Si_2Fe''_2O_7+H_2Al_2O_7$, and sapphirine he represents by the formula $Si_2Mg_2O_8+Al_6Mg_2O_{11}$.

Using the analyses cited by Tschermak, I find that all these minerals, with the possible exception of sapphirine, may be represented by the general expression

$$\begin{matrix} & \diagup O \\ Al-O & > R'' \\ & \diagdown SiO_4 \equiv R'_2, \end{matrix}$$

which is clearly and directly related to the formulæ already assigned to the micas and to margarite, and in which the mode of union of R'' with Al, when R''=Mg, suggests the common association of members of this group with spinel. In seybertite we have a mixture of $AlO_2R''SiO_4(MgOH)_2$ with $AlO_2R''SiO_4(AlO)_2$, R'' being partly Ca and partly Mg. In brandisite we have a similar constitution, with about one fourth of R'' replaced by H_2. In chloritoid, R''=Fe and $R'_2=H_2(AlO)$; and sismondine is similar, with R'_2 possibly replaced in part by Al. The compound $AlO_2MgSiO_4(AlO)_2$, found in seybertite, is a rough approximation to sapphirine, which mineral possibly has this composition plus some impurities as yet unidentified. A wider range of analyses is needed in order to establish these formulæ completely; but they seem to have distinct advantages over the formulæ proposed by Tschermak.

Conclusions.

All the micas, vermiculites, chlorites, margarite, and the clintonite group may be simply represented as isomorphous

* Zeitschr. Kryst. Min., iii, 496.

mixtures, every constituent being a substitution derivative of normal aluminum poly- or ortho-silicate. To the latter compound a structure may be assigned somewhat different in form from the one I have chosen, without affecting in any notable way the general system adopted. Upon this basis all the minerals named are reducible to the same general type, which accounts for observed isomorphisms, and for the relations of the micas to other species, with fewer assumptions of hypothetical compounds than are necessary under other known schemes of interpretation. In most cases the evidence is clear, direct and conclusive; in other cases, few in number, it is at present somewhat obscure. It may be claimed, without extravagance, that the formulæ have the merit of suggestiveness, and that they form a scientific basis for future research.

Washington, July 15, 1889.

ART. XLIX.—*The Probable Law of Densities of the Planetary Bodies;* by ROBERT HOOKE.

As far as the writer is aware no inquiry has ever been made into the connection between the diameters and mean densities of the planetary bodies, or if such inquiry has been made we have no record of the result arrived at; yet it is an interesting subject of investigation, and in the light of modern science it is to be wondered at that the subject has not attracted the attention of some investigator long before this. Some three years ago the attention of the writer was accidentally directed to this subject through the reading of Professor Newcomb's Popular Astronomy, and the result of the investigation, which is now given publicity for the first time, was quickly arrived at, owing to its very simple character.

In the absence of knowledge respecting the law of compressibility of matter as it exists in the planets, it was impossible to formulate the law of relative density of planetary bodies, of different diameters, from theoretical considerations. It was therefore necessary to determine the law by experiment alone, and the experiments must, from necessity, be the great natural ones presented by the planets themselves. It was necessary at the very outset of the investigation to adopt the hypothesis—that the planets were formed of the same material, or to put it more accurately—that the material which forms the principal part of the masses of the planets and their satellites would, when subjected to the same conditions of temperature and pressure, have the same density. Under this hypothesis the difference between the mean and surface densities of the earth was due alone to the compression to which the matter in the

interior of the earth is subjected, and the difference in the mean densities of the moon and the inner planets, to the different degrees of compression which has been produced by gravity in the interior of their masses. It was clear that, under this hypothesis, all of the bodies referred to would have the same surface density, provided they had reached the same physical condition, that is, the condition of solidification.

In the selection of the planetary bodies for the purpose of experiment, to determine the law of density, and also the bodies to which the law indicated may be applied as a test of its correctness, it was necessary to arrange the bodies of the solar system into two classes, the first to embrace those in which solidification had taken place, and which, under the adopted hypothesis, would have the same surface density; and the second, those bodies which had not reached the solid state, and in which there was an expansion of volume beyond that which they would have in the solid condition. It is clear that with the latter class the surface density would probably be different with each body. Therefore the bodies to be selected for the experiment, to determine the law connecting the density with the diameter, and also the bodies to which the law indicated must be applied as a test, must belong to the first class. Though the law in question, when determined, would not be applicable to the planetary bodies of the second class in their present condition, yet it could be applied to the computation of the ultimate diameters and mean densities of those bodies, that is, the diameters and mean densities they will have when they reach the condition of solidification.

The planetary bodies which were assumed to belong to the first class are the earth, the moon, and the planets Mars, Venus and Mercury, and probably also the satellites of the outer planets; and those belonging to the second class are the outer planets, Jupiter, Saturn, Uranus and Neptune, and also the sun. The evidence to justify the foregoing classification will be referred to in the concluding remarks of this paper.

In seeking for the law connecting the mean density with the diameter it was clear that no attempt should be made to find the law connecting directly the diameter with the mean density as a whole, but simply with that part of the mean density which was due to compression, or, in other words, to find the law connecting directly the diameter with the difference between the mean and surface densities. This made it necessary to adopt some value for the surface density of the bodies selected for the investigation, which were those in which the greatest accuracy had been attained in the determination of their mean densities and diameters, namely, the earth and the moon. And the value adopted for the surface density of the

two bodies, was the mean of the following values assigned to the earth's surface density by eminent authorities:

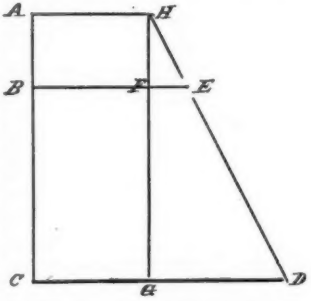
Laplace	2·50
Airy	2·56
Waltershausen	2·66
Mean of the above values	2·57

The values of the mean diameters and mean densities of the earth and moon used in the investigation, are as follows:

Mean diameter of the earth	7918	miles.
Mean diameter of the moon	2160	"
Mean density of the earth	5·66	that of water.
Mean density of the moon	3·42	" "

(All of the densities given in this article are relative to that of water, which is taken as 1.)

Let AB, on the diametral scale, represent the diameter of the moon, and AC the diameter of the earth; draw CD, on the scale of density, for the earth's mean density, BE for the moon's mean density, and AH for the density of a planetary body whose diameter is supposed to equal 0, and also for the surface density of the earth and moon; draw HG parallel to AC, and we have GD for the difference between the mean and surface density of the earth, and FE for the difference between the mean and surface density of the moon.



Adopting for AH, BE and CD, the values given above for the earth's surface density, the moon's mean density and the earth's mean density, respectively; and for AC and AB the values given for the respective diameters of the earth and moon, we perceive that the point E lies so nearly in a straight line connecting H and D, that it is highly probably that

$$FE : GD :: AB : AC.$$

From the above simple investigation we conclude that for planetary bodies of the same surface density, the *increase of the difference between the mean and surface density is proportional to the increase of diameter.* A test of the correctness of this conclusion will be made by applying the law to the computation of the mean densities of the inner planets from their assigned diameters.

To determine the value of the surface density of the earth and moon from their mean densities and diameters, we have, according to the indicated law of density, the following equations:

Make P the surface density of the earth and of the moon.
X the difference between the mean and surface density of the earth.
Y the difference between the mean and surface density of the moon.
$P+X$ the earth's mean density.
$P+Y$ the moon's mean density.
D the earth's diameter.
d the moon's diameter.
And k the difference between the mean and surface density of a planet of unit diameter, and we have

$$k=\frac{P+X-(P+Y)}{D-d} \tag{1}$$

and

$$P=P+X-Dk=P+Y-dk. \tag{2}$$

If we have another planet of the same class as the earth and moon, whose diameter is D′, and whose mean density is P+Z, we have, according to the law of density,

$$P+Z=P+D'k. \tag{3}$$

Substituting the proper numerical values in equations (1) and (2), and making the unit of diameter a mile, we get

$$k=\cdot000389, \text{ and } P=2\cdot58.$$

Dk and dk in equation (2) represent respectively that part of the earth's mean density, and that part of the moon's mean density, which is due to compression, and their numerical values are as follows:

$$Dk=3\cdot08, \text{ and } dk=0\cdot84.$$

The writer has tested the law of density by applying equation (3) to the planets Mars, Venus and Mercury. The following are the computed values of the mean densities of these planets, on the basis of the law in question, compared with the values which have been computed from the assigned values of their masses and diameters.

	Diameter in miles.	Value computed from law of density.	Value computed from assigned values of masses and diameters.	Mass (Sun=1.)
Mars	4211	4·22	4·17	$\frac{1}{3093500}$
Venus	7660	5·56	5·24 (?)	$\frac{1}{380000}$
Mercury	2992	3·74	4·56 (?)	$\frac{1}{7600000}$

The agreement of the computed and assigned values is very close in the case of Mars, but not so close in the cases of Venus and Mercury, yet the differences in the latter cases are probably not greater than the uncertainty of the assigned values; for the density of Venus as given in the Ninth Edition of the Encyclopædia Britannica (Art. Astronomy), is 1·03 that of the earth, which reduced to that of water, is 5·83. This value is based on a diameter of 7510 miles. Computing the mean density of Venus from this value of the planet's diameter, according to the law of density, comparing the result with the assigned value just given (5·83) and then taking the mean of the values given in the two comparisons, we have the following result:

	Values according to law of density.	Values computed from the assigned masses and diameters.
1st comparison	5·56	5·24
2d comparison	5·50	5·83
Mean of the above values	5·530	5·535

From the results of the foregoing test of the law of density, as applied to the planets Mars and Venus, the law may be said to be verified. The apparent discrepancy in the case of Mercury does not prove the inapplicability of the law to that planet, as the assigned value of the mass is still somewhat in doubt, and the doubt of the mass taken in connection with the uncertainty in the assigned value of the planet's diameter, may be sufficient to eliminate the entire discrepancy.

The foregoing tests include all of the planets to which the law of density is directly applicable, but, as before stated, it is quite probable that the satellites of the outer planets belong to the same class of bodies as that to which the inner planets and the moon have been assigned. The test cannot, however, be applied to these bodies for the reason that nothing certain is known of their diameters. The masses of the satellites of Jupiter have probably been determined with sufficient accuracy to give approximate values of their densities, provided the diameters of the satellites were accurately determined. The observed values of the diameters of these satellites are probably very much in error, as their apparent size would be increased by irradiation. But if we suppose the principal error in the observed values to be that due to irradiation, we may get some evidence of the applicability of the law of density to these bodies in the following manner: thus, if the errors in the determination of a set of values for the diameters of these satellites were those due to irradiation alone, the value determined for each satellite would probably be affected by the

same error of angular measure, and the error would be one of excess, so that, if we had the actual diameters of the satellites, and should subtract them from the observed diameters, the same difference would be obtained in the case of each satellite. The writer has computed the diameters of these satellites from the law of density, previously applied to the inner planets, and the most recent values of the masses of the satellites relative to that of Jupiter, and the following is a comparison of the computed with the observed values, the latter being those given in the ninth edition of the Encyclopædia Britannica:

	Observed diameter in miles.	Computed diameter in miles.	Difference in miles.
Satellite I	2352	1663	689
Satellite II	2099	1837	262
Satellite III	3436	2768	668
Satellite IV	2929	2214	715

It will be observed that the difference is about the same for each satellite except the second, the diameter of which computation makes larger than the first, but which observation makes smaller. With the single exception just noted, the result of the above test is confirmatory of the law of density and the theory that these satellites have solidified, and therefore, under the adopted hypothesis, have the same surface density as the inner planets and the moon. The apparent want of coincidence in the difference obtained for the second satellite does not disprove the law of density, or its applicability to these satellites, for the reason that several distinguished observers have observed this satellite to be larger than the first, as the value of its mass indicates it should be.* The values used for the masses of the satellites in the computation of the diameters above given are as follows:

	(Mass of Jupiter=1.)
Satellite I	0·000016877
Satellite II	0·000023227
Satellite III	0·000088437
Satellite IV	0·000042475

The mean densities of the satellites of Jupiter are, according to the above values of their masses and the computed diameters already given, as follows: satellite I, 3·22; satellite II, 3·29; satellite III, 3·66; satellite IV, 3·44.

* There is, in all probability, a serious error either in the observed value of the diameter, or in the assigned value of the mass of the second satellite of Jupiter, as it is very improbable that in the same system of satellites, a satellite of given volume should have a greater mass than one of larger volume.

An interesting application of the law of density, under the adopted hypothesis, is to determine, by computation, the ultimate diameters and mean densities of the sun and the outer planets, that is, the diameters and mean densities they will have when they have become solid like the earth and moon. That they will ultimately reach this condition, if they undergo no other change except that due to the radiation of heat, cannot be questioned. The application referred to will, first, be made to the sun.

Let d be the diameter of an hypothetical planet of the class to which the inner planets have been assumed to belong, and whose mean density is $2P$, P being its surface density, and let the diameter of this hypothetical planet, which is found by the law of density to be 6632 miles, be taken as the unit of diameter, and its mass as the unit of mass. Now if we make M the mass of another planetary body whose diameter is $1+x$, we have, by the law of density

$$M=(1+x)^3\frac{x+2}{2}$$

or
$$2M=2+7x+9x^2+5x^3+x^4.$$

Making M the ratio of the mass of the sun to that of the hypothetical planet (610400) which is taken as the unit of mass, and finding the root of the equation, we have

$$1+x=1+31{\cdot}9928=32{\cdot}9928,$$

from which we get the following value for the ultimate diameter of the sun, in miles:

$$32{\cdot}9928\times 6632=218808;$$

and for the sun's mean density we have from equation (3)

$$218808\,k+P=87{\cdot}69.$$

The same application of the law of density has been made to the outer planets, and the following are the results obtained for their ultimate diameters and mean densities, the list being headed with the values for the sun for the purpose of comparison:

	Ultimate diameters.	Ultimate mean densities.
Sun	218,808 miles.	87·69
Jupiter	37,183	17·04
Saturn	27,128	13·13
Uranus	16,552	9·00
Neptune	17,220	9·28

The foregoing results are based on the following values of the masses of the sun and of the planets mentioned:—

Sun	326800	times	that	of the earth.
Jupiter	$\frac{1}{1047\cdot88}$	"	"	sun.
Saturn	$\frac{1}{3501\cdot6}$	"	"	"
Uranus	$\frac{1}{22600}$	"	"	"
Neptune	$\frac{1}{19300}$	"	"	"

We may apply the foregoing values of the ultimate mean densities of the sun and outer planets to the computation of the present surface densities of these bodies; but the results must be based on the following supposition, namely, that in the contraction of the sun or planet from its present diameter to that assigned above for its ultimate diameter, the mean motion of the molecules at different distances from, and in the direction of the center of the mass, would be proportional to their distance from the center. According to this supposition the ratio of the mean to the surface density will remain constant during the contraction.

Let d be the present mean density of the sun;
D its ultimate mean density;
S its ultimate surface density,
and X its present surface density,

then $$X=\frac{dS}{D}=\frac{1\cdot444\times2\cdot58}{87\cdot69}=0\cdot0424.$$

Applying the same method of computation to the outer planets we get the following values for their present surface densities:—

	Present surface density.	Ratio of mean to surface density.
Sun	0·0424	34·0
Jupiter	0·208	6·6
Saturn	0·147	5·1
Uranus	0·365	3·5
Neptune	0·321	3·6

These results show that the surface density of the sun is, as would naturally be expected, much less than that of either of the outer planets. They also show that the surface densities of Uranus and Neptune are greater than the corresponding values for Jupiter and Saturn; this is also what might have been expected, as the former planets being smaller than the latter have made greater progress in the process of condensation. The only anomaly in the above results is in the case of Saturn, which, being smaller than Jupiter, might have been expected to have a somewhat greater surface density, though the results show that the surface density of the former planet is less than that of the latter. But when it is considered that Saturn presents a most striking anomaly in another feature,

that of being encircled by a system of rings, we may fairly conclude that the results given above probably represent truly the relative, if not the actual, surface densities of the outer planets and the sun.

The following interesting inquiry naturally presents itself in considering the law connecting the mean densities of the planets with their diameters: What, under the law connecting the mean density with the diameter, should be the law connecting the density with the pressure? The relation of density to pressure, as deduced by the writer from the law of the planets' densities, is, that *the increase of the square of the density minus the density, is proportional to the increase of pressure;* thus, if D is the density and P the pressure, the increase of D^2-D is proportional to the increase of P. The relation of density to pressure, according to Laplace's hypothetical law of density within the earth, is, that the *increase of the square of the density is proportional to the increase of pressure.* For very great condensations the two laws would give approximately the same results.

The writer believes that all eminent observers who have in late years directed their attention to the examination of the surfaces of Jupiter and Saturn, agree that these planets present features widely different from those presented by the inner planets, and that on account of the rapid changes taking place on their surfaces, the interior must be the seat of enormous activity, which can be ascribed to no other cause than to a very high temperature resembling that of the sun. Could the planets Uranus and Neptune be examined from a distance not greater than that which separates us from Jupiter, they would in all probability present features similar to those of the latter planet. We are therefore justified by observation in assigning the outer planets to a different class from that to which the inner planets belong. The adopted hypothesis and the law of density herein indicated, require that the planets Jupiter and Saturn be not only in a fluid condition, but that their present volumes are from 12 to 17 times greater, respectively, than that which they will have after solidification has taken place.

Of the improbability of the law of density, herein indicated for the inner planets, being true, the writer has not been able to obtain the slightest evidence, based on accurate knowledge (the greatest disagreement in the test to which the law has been subjected, being in the case where there was the greatest uncertainty in the values with which those derived from the law were compared); but on the other hand all of the evidence which has any bearing on the subject at all tends to the confirmation of the law. For instance the most recent changes in

the assigned values of the masses of the planets Mars, Venus and Mercury, based presumably on more accurate data or improved methods of computation, have been in the direction of the values which the law in question would from their observed diameters have assigned to them. For the full confirmation of the law it may yet be considered necessary by the cautious investigator to have some additional proof of its correctness. In what direction it is best to seek for additional proof of a decisive character it is difficult to say. Could the law of increase of density within the earth which would result from the derived law of the planets' densities, be deduced, evidence of the correctness of the law might be found in the same way that Thomson and Tait have in their treatise on Natural Philosophy, Part II, section 828, sought for evidence of the correctness of Laplace's hypothesis. It is to be hoped that more accurate determinations may be made of the masses and diameters of the planets Venus and Mercury, and of the satellites of Jupiter. It is in this direction that the writer has most hope for the full confirmation of the law in question.

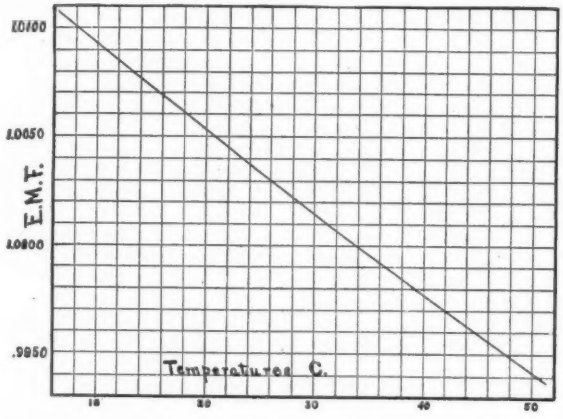
ART. L.—*An improved Standard Clark Cell with Low Temperature-Coefficient;* by H. S. CARHART.

THE best form of Clark cell hitherto made is that of Lord Rayleigh, described in the Philosophical Transactions for 1885. The objections to this form are that the temperature-coefficient is not the same for all cells, as is shown in Lord Rayleigh's paper, and it is so high as to introduce a very troublesome and uncertain error because of the difficulty of ascertaining the exact temperature of the cell; second, it is not so constructed mechanically as to prevent the mercury from coming into contact with the zinc when the cell is subjected to violent jars in transportation; thirdly, a great chemical defect is the facility with which local action takes place between the zinc and the mercury salt. I might add that the mercurous sulphate, purchased by Lord Rayleigh, evidently contained considerable salt in the mercuric form, as is shown by its turning yellow on mixing with the zinc sulphate solution.

All these difficulties I have, at least in large measure, overcome. Respecting the materials, the greatest care is required to secure and maintain cleanliness and purity in their preparation. The mercury must be distilled *in vacuo* after being cleaned by chemical means. The zinc sulphate should be free from iron as well as other impurities. The mercurous sulphate can be made almost or quite free from the mercuric form by

using plenty of mercury; keeping the temperature down to the lowest point at which action will take place; and letting the mixture of salt, acid and metallic mercury stand for some time. I have made in this way a salt that remains white, not only when the free acid is all washed out, but when mixed with the standard zinc sulphate solution. Further, it remains white in the cell indefinitely if it is not exposed to a bright light.

Hitherto the importance of the local action going on in a Clark cell appears not to have been appreciated. It accounts for some of the differences in temperature-coefficient and leads to some more serious results in some cells. The zinc replaces mercury when in contact with the mercury salt. This amalgamates the zinc, producing a slight change in the E. M. F.;



and then the amalgam is liable to creep up to the top of the zinc where it attacks the solder. The copper wire is thus sometimes loosened. The zinc sulphate follows up and the cell may thus be short-circuited by the zinc and the copper wire. Upon taking down one cell, which was perhaps a year old, I found that the zinc had been removed from the rod at the surface of the liquid and had been deposited again upon the rod at the surface of the mercury salt, in a solid frill around the zinc. The copper wire in this cell became entirely detached, partly because of the expansion upward of the marine glue, which brought a severe strain upon the wire.

The local action then increases zinc sulphate in the cell at the expense of the mercury sulphate and amalgamates the zinc rod. I have become convinced by some experiments extending over several weeks that this substitution process goes on

only when the zinc is in contact with the solid mercury salt. The mercurous sulphate is only slightly soluble in a saturated solution of zinc sulphate. I prevent local action, therefore, by keeping the zinc and the mercury salt out of contact. The same device operates to raise the E. M. F. about 0·4 per cent. The following table exhibits the observed and calculated values of the E. M. F. of cells No. 17, 112, 113 in terms of No. 1 [old style] at 20° C.:

No. 17.			No. 112.		
Temp. C.	Observed.	Calculated.	Temp. C.	Observed.	Calculated.
8°·3	1·0108	1·0106	5°·1	1·0124	1·0125
8·5	1·0103	1·0105	10·6	1·0106	1·0103
9·3	1·0104	1·0102	12·5	1·0098	1·0096
11·8	1·0093	1·0092	15·2	1·0087	1·0086
13·8	1·0084	1·0085	19·5	1·0069	1·0069
15·0	1·0080	1·0080	21·2	1·0062	1·0062
18·1	1·0069	1·0068	31·1	1·0024	1·0024
19·4	1·0064	1·0063			
19·9	1·0062	1·0061	No. 113.		
20·3	1·0060	1·0059			
20·8	1·0054	1·0057	5·1	1·0124	1·0125
21·1	1·0057	1·0056	10·6	1·0106	1·0104
21·6	1·0054	1·0055	12·5	1·0098	1·0097
22·4	1·0050	1·0052	15·2	1·0088	1·0087
23·3	1·0048	1·0048	19·5	1·0070	1·0070
25·1	1·0044	1·0041	21·2	1·0062	1·0063
26·4	1·0035	1·0036	31·1	1·0025	1·0025
30·2	1·0019	1·0022			
33·1	1·0014	1·0013			
39·1	0·9991	0·9989			
41·7	0·9980	0·9979			
50·4	0·9949	0·9947			
52·7	0·9939	0·9940			

Cell No. 1 was always very near 20° C., and the reduction to that temperature was made by means of Lord Rayleigh's reduction coefficient, ·00077 per degree C.

The equation for the E. M. F., derived from the observations on No. 17, is

$$E' = E[1 - ·000387(t-15) + ·0000005(t-15)^2].$$

The calculated values for the three cells were all obtained by this formula. The change for one degree C. is, then, the following linear function of the temperature:—

$$-·000386 + ·000001(t-15).$$

The temperature-coefficient ranges from ·000361 at 0° C. to ·000376 at 25° C., and to ·000361 at 40°C. At the highest observed temperature in the preceding table it was only ·000348. The curve of E. M. F. with temperatures as abscissas is clearly concave upward, indicating a fall in the temperature-coefficient

with rise of temperature. The change is, however, so small as to be quite negligible within the range of temperature to which a normal element is subjected in practice. Lord Rayleigh's cells show a change in the temperature-coefficient directly the reverse of the above; that is, the coefficient increases by a very appreciable quantity with rise of temperature. For his No. [36] the coefficient ranged from ·000556 at 0° C. to ·00101 at 25° C., if his equation holds true for the higher temperature.

In making comparisons of E. M. F. I have used Lord Rayleigh's method, slightly modified, by means of which a difference of one ten-thousandth part is observed directly and with the greatest ease. In fact a difference of half that amount is easily measured. A comparison of half a dozen cells can be made in as many minutes without difficulty.

As to polarization, these cells show none with external resistance greater than 30,000 ohms. At 30,000 ohms the polarization is just discernible; and with 10,000 ohms it amounts to only one ten-thousandth part in five minutes. This fall in E. M. F. is less than the accidental differences between different cells in general, and much smaller than the almost unavoidable errors due to ignorance of the real temperature of the cell. If the cell is not closed on less than 10,000 ohms resistance, and only for a few minutes, the polarization may be entirely neglected.

As indicating the uniformity attained, the following relative values of the E. M. F. of six cells, only four days old, may be given: 9048, 9049, 9049, 9048, 9046, 9043. The last one was still approaching the others when last observed. Six cells of later construction gave the following relative values when less than two days old: 9182, 9182, 9182·5, 9182, 9182, 9182·5. The two sets of numbers do not represent at all the relative values of one set as compared with the other.

It will be seen from the table that Nos. 112 and 113 never differ by more than one part in ten thousand at the same temperature.

ART. LI.—*Pseudomorphs of Native Copper after Azurite, from Grant County, New Mexico;* by W. S. YEATES.

DURING the month of April of this year, Mr. J. A. Lucas of Silver City, Grant County, New Mexico, sent to the U. S. National Museum a specimen with this brief note:—"From the 'Copper Glance' and 'Potosi' copper mines, Grant Co., New Mexico. This ore is found in all imaginable shapes and sizes from 1 oz. to 70 lbs." The specimen was referred to the

writer, for identification. It had the appearance of copper; but it was very brittle, and its specific gravity was much too low for ordinary metallic copper, a fragment yielding only 4·15. The surface was, in part, made up of what appeared to be tabular crystals, reminding one, in general form, of the azurite crystals from the Copper Queen mine, in the adjoining county of Cochise, Arizona, though the surfaces of the planes were stippled, rendering only approximate measurements of the angles possible. The surface of the specimen was coated with a white clay-like substance, the most of which had been cleaned off with a knife. This substance was found to be kaolin. An examination of the fresh fracture with a lens showed that the kaolin not only coated the surface, but that it was intimately mixed with the copper-like particles, producing a granular fracture, and giving rise to the stippling on the crystal surfaces. A fragment under the pestle in an agate mortar was reduced to powder, the metallic grains, which had been proved, before the blowpipe, to be copper, segregating together, and marking the mortar and pestle with bright shining streaks. The copper being so finely divided, it was now clear why the specimen was brittle, and why it had so low specific gravity. If the copper was, as it appeared to be, a pseudomorph after azurite, the latter must have lost its carbonic acid and water in the presence of some reducing agent, probably volcanic gases thrown up from below, leaving the copper in a spongy state, upon which the kaolin was deposited, and forced by pressure, while in a soft, semi-liquid condition, into the pores of the sponge.

With a view to determining whether or not this was true, Mr. Lucas was requested to furnish additional specimens for study, which he kindly did. Among them, were several, which, he informed the writer, had been cleaned with brush and water and a knife; the others were in the condition in which they were taken from the mines. These last appeared, externally, to be flattened nodules of kaolin, sometimes colored reddish brown. On the two opposite flattened sides were slickensides—evidence of the pressure, which had forced the kaolin into the copper sponge. With dental instruments, the writer exposed a fine group of the crystals of one of the nodules, the most prominent crystal being almost perfect, and simulating in form those azurite crystals flattened parallel with the plane $-1\text{-}i$, so common at the Copper Queen mine. By measurement with a contact goniometer the angle between the broad plane and an adjacent plane, on the copper crystal, was found to be identical with that of $-1\text{-}i \wedge 2$ on a fine Copper Queen azurite crystal in the Museum collection.

Additional circumstantial evidence may be found in the fact

that the azurite crystals from the "Anson S." mine in Grant county, a neighbor to the "Copper Glance" and "Potosi" mines, are generally covered with kaolin, when found; and this is true of many of those from the Copper Queen mine.

In a letter, Mr. Lucas says:—"At a depth of about 40 ft., the green and blue carbonates run out, and the brittle copper comes in, getting better and purer, and increasing in quantity."

In conclusion, the writer wishes to tender his acknowledgments to Mr. Lucas, for his generous courtesy and aid.

U. S. National Museum, July 2, 1889.

Art. LII.—*Note on the Relation of Volume, Pressure and Temperature in case of Liquids;** by Carl Barus.

Many experiments made with alcohol, ether, para-toluidine, diphenylamine, paraffine, thymol, and less completely with naphthaline, vanilline, azo-benzol, α-naphtol, monobrom camphor, benzoic acid, caprinic acid, palmitic acid, and monochloracetic acid, show that if temperature and pressure vary linearly at a mean rate of about $\cdot 11^\circ$ C. per atmosphere there will be no change of volume. My temperatures lie between 30° and 300°, and the pressure between 20 atm. and 500 atm. By judicious extrapolation (enclosure of the experimental data, volume, pressure, temperature, between two mathematical functions, one of which is necessarily above and the other below the observed values; whereas both of the said functions fall within the limits of error, within the interval of observation), the probable contours can be computed to 1000 atm., with results accentuating the above law. The linear relation was predicted from theoretical considerations by Dupré (1869) and by Lévy (1884)—considerations soon proved to be inadequate by Massieu, H. F. Weber, Boltzmann and Clausius. Ramsay and Young (1887) established the relation in question experimentally for vapors, but not, I think, very fully for liquids decidedly below their critical points. Reasoning from these data Fitzgerald (1887) investigated the consequences of the law, viz: (1) specific heat under constant volume is a temperature function only; (2) internal energy and entropy can be expressed as a sum of two terms one of which is a volume function only and the other a temperature function only. Thus Ramsay, Young and Fitzgerald arrive substantially at the same position from which Dupré and Lévy originally started.

* The present communication has the assent of Mr. Clarence King, at whose suggestion and under whose instruction the work was done.

My own results were developed quite independently of the earlier work, and they apply emphatically for liquids. Thymol and toluidine were even under cooled 25° and 15° below their respective melting points. My range of pressures is therefore 6 or 7 times as large as that of Ramsay and Young. Thus my work, supplementing the researches of the English chemists, is not superfluous. Water seems to be a notable exception.

I found furthermore, that the pressure necessary to solidify a substance is, cæt. par., decidedly in excess of the pressure (positive or negative, external) at which it again liquifies. Here therefore is an exceedingly simple, and hence a type lag phenomenon. Making extensive use of it, I am led to results bearing directly on all lag phenomena, and beyond this on the molecular structure of matter in general. Data are in hand for paraffine, naphthaline, palmitic and chloracetic acids.

Operating above 100° C., I observed that (liquid) water at a pressure of 20 atm. and a temperature of 185° attacks ordinary lead glass so rapidly that in very fine capillary tubes the contents become opaque and solid in about an hour. During this action the compressibility at 185° gradually and regularly increased to a final value (200×10^6), about 3 times the original value (77×10^6). At the same time the isothermal volume of the silicated water decreased fully 13 per cent of its original bulk. So large a contraction will probably not be unaccompanied by rise of temperature.

Finally, in case of mercury, the simultaneous decrements of electrical resistance, r, and volume, v, due to pressure (0 to 400 atm.), were found to be proportional to each other. Approximately $\delta r/r = 10\ \delta v/v$. This result initiates a new method of attacking the above thermo-dynamic problems, and it has already led ulteriorly, to results of electrical interest.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Colloidal Cellulose.*—By treating filter-paper, previously purified with hydrochloric and hydrofluoric acids, or the finest quality of carded cotton, carefully dried, with sulphuric acid of 50° B. in the cold, Guignet has obtained a colloidal form of cellulose which, when the acid has been completely removed by washing, is readily soluble in pure water. To ensure a complete removal of the acid the last washings are performed with alcohol and the product is dried at the lowest possible temperature. Before washing, the cellulose forms a transparent gelatinous mass which is not affected by contact with a large excess of

acid, but at 100° is rapidly converted into dextrin. The solution of colloidal cellulose in water is slightly milky, is readily filtered, deposits no precipitate even after several hours' standing and is not altered by boiling. It is slightly orange-yellow in color and rotates the polarized ray slightly to the right. Small quantities of sulphuric or of nitric acid, of sodium chloride or sulphate, of lead acetate, etc., precipitate it, as is the case with other colloids. Alcohol in large excess acts similarly. Colloidal cellulose does not reduce Fehling's solution, is not colored by iodine, and differs from the achrodextrins in being precipitated by salts added in small quantity. A solution poured on a marble surface previously rubbed with vaseline and well polished, forms brilliant semi-transparent pellicles which swell up in water and then dissolve. An immersion in sulphuric acid of 60° B. for a short time, or in acid of 55° B. for a longer time, causes the colloidal cellulose to become insoluble in water, a small quantity of dextrine being at the same time formed. Nitric acid converts it into nitrocellulose. Parchment paper, thin and prepared probably by means of a somewhat weak acid, yields colloidal cellulose to boiling water. But thick paper, presumably treated with a stronger acid is insoluble. Parchment paper therefore may be regarded as a cellular tissue, the pores of which have been filled up with colloidal cellulose.—*C. R.*, cviii, 1258–1259, August, 1889. G. F. B.

2. *Watts' Dictionary of Chemistry*, revised and entirely rewritten by H. Forster Morley and M. M. Pattison Muir; assisted by eminent contributors. In four volumes, vol. i, 752 pp. 1888, vol. ii, 760 pp. 1889. London and New York. (Longmans, Green & Co.).—The first edition of Watts's Dictionary of Chemistry, the publication of which was begun in 1863, will always rank as one of the monumental works of the science. The fifth volume was completed in 1868 and since then a series of supplementary volumes has been issued, the last (1881) being the ninth volume of the entire work. The immense growth in the subject of chemistry in all its branches, however, had already carried it well nigh beyond the possibilities of a single work.

Sometime before the death of Mr. Watts in 1884, he had undertaken to prepare a new edition of the Dictionary, but his too early death interrupted the work before more than a beginning had been made and the labor passed to the hands of the present editors. As this new edition is to be limited to four volumes of about 750 pages each, the subjects treated of have been restricted in the first place to those in chemistry proper, excluding the "allied branches of other sciences" which found place in the first edition, and also reserving technical chemistry for a companion volume to be published under the editorship of Professor Thorpe. Moreover it has been necessary to adopt a rigid system of abbreviation and condensation so as to compress the almost unmanageable mass of material into the smallest possible space. Half of the work is now in the hands of chemists and it is needless to say that the great labor has been performed in a faithful, accurate

and masterly manner—in fact the names of the editors and of the special contributors are a sufficient guarantee of the excellence of the whole. The unavoidable brevity of treatment will sometimes be regretted and the amount of abbreviation may seem to an occasional reader a serious drawback; but this disappears upon frequent use. Subjects in general chemistry have not been neglected and we note, for example among the numerous excellent articles, one by Mr. Muir upon atomic and molecular weights (24 pages), others by Dr. W. Ostwald on affinity, by Prof. E. Ray Lankester on bacteria, by Prof. J. J. Thomson on states of aggregation, and chemical equilibrium, by Prof. T. E. Thorpe on the atmosphere, combustion and flame, by Prof. F. W. Clarke on elements and Prof. Ira Remsen on equivalency and formulæ. The completion of the entire work will be looked forward to with much interest, and the gratitude of chemists is due to those who have been willing to undertake so great a labor. It is a matter of regret that the author who projected the work and whose name it bears could not have lived to see its completion.

3. *A Treatise on the Principles of Chemistry;* by M. M. PATTISON MUIR. Second edition. 490 pp. 8vo. Cambridge, 1889, (University Press).—The second edition of this philosophical work will be warmly welcomed by all who have become acquainted with the excellence of the first edition published five years ago (see this Journal, vol. ix, 255). Rapid progress has been made during this period in the development of chemical principles, and this has necessitated the fundamental revision of much of Book I on Chemical Statics, and the entire rewriting of Book II on Chemical Kinetics, especially that part dealing with chemical affinity. The chemical student will find the modern ideas on both branches of the subject presented in a very satisfactory and instructive form.

4. *Definitions adopted by the International Congress of Electricians.*—Professor MASCART communicated to the French Academy the following definitions proposed by the late Congress of Electricians:—

Unit of Work, the Joule. Equal to 10^7 C.G.S. units. It is the energy equivalent to the heat produced by one ampere through one ohm in one second.

Unit of Power, the Watt. Equivalent to the power produced by one Joule in one second and is equal to 10^7 C.G.S. units.

The Quadrant is the unit for coefficient of induction. The quadrant is expressed as a length and is equal to 10^9 centimeters.

The frequence of an alternating current is the number of periods per second.

The actual strength of an alternating current is the square root of the mean square of the strengths.

The actual electromotive force is the square root of the mean square of the electromotive forces.

The apparent resistance of the current is the factor by which one must multiply the actual strength in order to obtain the actual electromotive force.

The practical unit of light is called the decimal light and is the one-twentieth of the absolute standard defined by the International Conference of 1884.

The *bougie décimale* is sensibly equal to the English candle—or to one-tenth of the Carcel burner.—*Comptes Rendus*, Sept. 2, 1889, p. 393. J. T.

5. *Lightning and the Eiffel Tower.*—The Eiffel tower was struck by lightning on the 19th of August, 1889, and Professor Mascart gives a short account of the occurrence in a note to the French Academy. The tower is protected by a central rod at its summit and by rods projecting from the balustrade of the third platform. The lightning discharge passed down the central lightning rod. Various subsidiary discharges were noticed at different parts of the tower by the four attendants who were on the tower at the time. The cloud which touched the top of the tower was vividly lighted. None of the four persons on the tower received the slightest shock although the iron work of the tower was wet by the shower. Professor Mascart remarks that security to life is absolute on a tower of this construction.—*Comptes Rendus*, Aug. 26, 1889, p. 355. J. T.

6. *Transmission of power by Electricity.*—M. Deprez states that power has been transmitted regularly for some months from a fall of water to the village of Bourganeuf, a distance of 14 kilometers. The line which transmits the current is of *bronze silicieux* (pure copper). The wire is 5mm. in diameter, uninsulated and is supported on wooden poles furnished with porcelain insulators. The generator and motor are ring-formed and furnish 100 nominal horse power. The electromotive force of the generator is 3000 volts.—*Comptes Rendus*, Sept. 2, 1889. J. T.

7. *Dissipation of negative electric charges by sunlight and daylight.*—J. Elster and H. Geitel show that the dissipation of negative charges can be caused not only by the ultra violet rays, but also by sunlight and diffuse daylight.—*Ann. der Physik und Chemie*, No. 9, 1889, p. 40. J. T.

8. *Photography of the invisible portions of the Solar Spectrum.*—M. Ch. V. Zenger states that one prism of rock salt combined with two rectangular prisms of *anéthole* gives between the lines A and D six times the dispersion of one prism of rock salt. The arrangement is called by Zenger a parallelopiped of dispersion.—*Comptes Rendus*, Sept. 9, 1889, p. 434. J. T.

9. *The Carbon Spectrum.*—H. Kayser and C. Runge have examined the band spectra of carbon given by the electric light between carbon terminals in order to see if these band spectra coincide with those of cyanogen. The results of their measurements force them to conclude that the identity of the cyanogen spectrum and the carbon spectrum has not been made out. They give an empirical formula for the arrangement of lines in the band spectra of carbon, and state their belief that the true law of the arrangement of lines in the band spectra can only be arrived at from a theoretical discussion of the law of molecular move-

ments. A table of wave-lengths of carbon bands and cyanogen bands is appended to their article.—*Ann. der Physik und Chemie*, No. 9, 1889, pp. 80–90. J. T.

II. Geology.

1. *Movement of the Upper ice of Glaciers over the Lower.*—Mr. Forel describes cases of the over-riding of the lower beds of ice by the upper in the glaciers of the Alps. (1) A cavern excavated in the spring for several years past, in the vertical wall in the left side of the Glacier des Bossons, at Chamouni, at the level of the lateral moraine (for the gratification of tourists) becomes raised, as the ice moves on, to a higher level during the summer, and in the following spring, it is some 25 to 30 meters above its former level, at the middle of the ice-wall; and during the following year the remains of the ice-cave reach the upper surface of the ice. (2) At the lower extremity of the Glaciers de Fée Inférieur, the Alallin, and the Rhone, in 1884 and of Zigiorenove in 1886, the slipping or over-riding of the upper beds has been observed to take place along planes of cleavage, which planes are those of the lamellar structure or of the blue bands. (3) A small frontal moraine, was formed in 1884 at the glacier of Hochbalm, by the slipping of a bed of white ice over an old bed of debris-covered ice. (4) Horizontal earthy bands of interior moraine are formed at the terminal extremity of certain glaciers as that of the Rhone in 1870, 1871, the Trient in 1882, and the Fée Supérieur in 1884. This movement takes place, says Mr. Forel, not by the deformation of the plastic mass, but by the slipping of one bed on another along planes of cleavage, which planes are those of the lamellar structure of the glacier. He observes that the facts serve to explain (*a*) the difference of velocity between the upper and lower beds; (*b*) the diminished rate of flow at the lower extremity of a glacier; (*c*) the re-appearance at the surface of bodies buried in the interior of the glacier; (*d*) the preservation of the thickness of the ice at the lower extremity, notwithstanding the annual loss from melting.

2. *The Ice Age in North America, and its bearings on the Antiquity of Man*, by G. Frederick Wright, D.D. 622 pp. 8vo, with many new maps and illustrations. New York, 1889. (D. Appleton & Co.)—Prof. Wright has for many years, as the readers of this Journal know, been an investigator of the Drift phenomena of North America. In 1886 he added to his knowledge of facts from the glaciated regions others from the study of existing glaciers in Alaska. Hence his right to speak with a degree of authority on the events of the Ice-age and formulate conclusions meriting consideration. He has made a work of great interest; and through the liberality of his publishers, one of special excellence in its illustrations and typography.

After general remarks on the structure of glaciers, the volume gives an account of the glaciers of the Pacific Coast from Cali-

fornia to Alaska, dwelling at length on the Muir glacier which was the one the author investigated. It next treats of the Greenland ice, in whose features the condition of the Ice-age are most nearly exhibited, and then of glaciers in Europe and other lands. After this introduction, extending over more than a hundred pages, the evidences and events of the Ice-age in North America are discussed at length with some account also of the facts from Europe. The position of the southern limit of the ice is described in detail and partly from personal observations, especially in Pennsylvania, Ohio, Indiana and Dakota. Afterward, the effects produced by glacial erosion and transportation are reviewed, the former including, in the author's opinion, many lake basins as well as river channels, and the latter comprising accumulations of debris into drumlins and kames. Other chapters treat of the buried valleys and extensive changes in river courses as a result of glacial depositions,—changes that led to the production of many of the waterfalls of the country, even the largest; the geographical march of vegetable and animal life consequent on the advance and retreat of the glacier; the cause of the glacial climate, under which subject Croll's theory is considered and no theory is found to be satisfactory; the date of the Ice-age; the antiquity of man. It is shown pretty satisfactorily that according to evidence from the rate of erosion at Niagara Falls, the St. Croix Falls, and from other facts, that the Ice-age probably closed from 8,000 to 10,000 years back, and it is inferred, from less clear proof, that only 15,000 to 25,000 years have elapsed since it began. The chapter on human relics in America reaches the conclusion that the earliest of them occur in deposits of the Glacial era, some thousands of years before its close.

An Appendix contains the views of Mr. Upham on "the probable causes of glaciation."

With regard to the Glacial period there is on several points wide diversity of opinion among geologists, and much is yet to be learned, and some of the conclusions of the author will find opponents. But the work, nevertheless, is a valuable review of the marvelous events of the Ice-age.

3. *Annual Report of the Geological Survey of Arkansas for* 1888, JOHN C. BRANNER, State Geologist. Vol. II. *The Neozoic Geology of Southwestern Arkansas*, by Robert T. Hill, Assistant Geologist.—The whole breadth of southern Arkansas and more than half of middle and northern are underlaid by Cretaceous and later deposits—a part of those of the great Mississippi bay of the Cretaceous and Eocene-Tertiary periods. The Report of Professor Hill treats of the distribution and stratigraphical features of the Cretaceous, Tertiary and Quaternary deposits in the southwestern portion of the State, the term Neozoic as used by him including the uppermost Jurassic, Cretaceous, Tertiary and Quaternary strata of the region. The Cretaceous formation has two divisions, as already explained by Mr. Hill* in this Journal,

* Vol. xxxiv, 287, 1887; xxxvii, 282, 1889.

the Lower or Comanche Series and the Upper or Exogyra costata series; and the lower of these has three divisions, the Trinity (or lowest), the Fredericksburg and the Washita. The author describes the several divisions, both of the Lower and Upper Cretaceous beds in detail, mentions the characteristics fossils and gives their distribution. Chalk, of varying purity, with and without flints, is described as constituting a large part of the Cretaceous of Arkansas as well as Texas, and especially of the Upper Cretaceous. In the middle portion of the latter in Arkansas, a bed of chalk is 500 feet thick. It is largely composed, according to the observations (for the Arkansas Survey) of Mr. J. S. Diller, of foraminifers, among which the genera Textularia and Globigerina predominate.

The Tertiary strata are chiefly those of the Lignitic series, the Eo-lignitic of Heilprin. The beds are treated of in the report under the local names of the Camden series, and the Cleveland County red-lands. The latter overlie the Camden series and contain Claiborne fossils. The Arkadelphia shales are at the base of the Camden. Professor Hill also describes the Quaternary beds. In later chapters he treats of the economical geology of the region—its iron ores, marls and chalk, its forests and its mineral springs.

The report contains also a chapter on the northern limit of the Mesozoic rocks in Arkansas by Dr. O. P. Hay, giving local details, and another on the manufacture of Portland cement, by J. C. Branner.

4. *Jurassic Plants from Kaga, Hida, and Echizen, (Japan);* by MATAJIRO YOKOYAMA. Journal of the College of Science, Imperial University of Japan, Vol. III, Part I, Tokyo, Japan, 1889.—This paper was announced three years ago in the Bulletin of the Geological Society of Japan (Part B, Vol I, No. 1, Tokyo, 1886), and a provisional list of the species given, of which the author then enumerated 54. Thorough study has reduced the number to 49, four of which had already been described by the late Dr. Geyler from collections made by Dr. J. Rein in the valley of the Tetorigawa, probably at Shinamura. This locality has since been thoroughly re-investigated together with six others. The flora shows a decided predominance of ferns, in which respect it differs from the Mesozoic floras of India and of Siberia and agrees better with that of the Yorkshire Oolite, but taking other elements into consideration it seems upon the whole to have the closest affinities with the Brown Jura of Siberia. The author refers the Japanese deposits to the Bathonian stage of the Lower Oolite. The paper is illustrated by fourteen litho-plates, in which the figures are as good as those of many European works. A second paper is promised on the Jurassic plants of the northern part of Shinano. L. F. W.

5. *The Rivers and Valleys of Pennsylvania*, by WM. MORRIS DAVIS, Nat. Geogr. Mag. i, No. 3, p. 103.—Professor Davis describes in this elaborate paper, the general topography of Penn-

sylvania with reference to the courses and kinds of valleys, their methods of origin as determined by the stratigraphic and other conditions, and illustrates the subject with sections, diagrams, views and maps.

III. Botany and Zoology.

1. *Die natürlichen Pflanzenfamilien.*—A. Engler and K. Prantl. Part 2, six sections, in all 1024 pages, with 3537 figures. (Leipzig, W. Engelmann, 1889).—The early signatures of this work have been already noticed in this Journal. Following the current fashion of "Subscription Books," the different signatures are scattered about among the different subjects in such a way that a subscriber must get the whole work, in order to secure what he may chance to want. And so we have part, or volume, second, before volume first. But this cannot be said to be a real hardship when the whole work must be regarded as indispensable to any botanical library. The editors have had numerous and able collaborators;—Graf zu Solms, Ascheron, Drude, Buchenau, Gürke, Häckel, Petersen, Pax, Pfitzer, and others, but they have themselves done a large portion of the work.

The volume begins with the treatise on the Gymnosperms by the lamented Eichler, and the Monocotyledonous orders follow in succession, closing with Orchidaceæ. Drude has a very interesting chapter on palms, Engler has treated of the following orders among others, Araceæ, and Liliaceæ, while Orchidaceæ naturally fell to Pfitzer. In treatment the authors have followed one plan throughout, namely, that of giving with illustrations, everything of importance in regard to the structure and relations of the typical plants of the respective orders, presenting necessary details regarding the genera. There appears to be in all parts of the work a general desire to present the more important facts relative to the plants which possess economic interest, and most of the more striking figures have been prepared with reference to this. The typography and illustrations are worthy of the text.

G. L. G.

2. *Guide pratique pour les travaux de Micrographie*, par Beauregard et Gallippe. Paris, 1888, pp. 901.—This work is the second and much improved edition of a hand-book of general utility in laboratories where the microscope is used. It presents a wide range of subjects, and most of them are well treated, but there is great inequality in their presentation, and some of them have no interest from a botanical point of view. For instance more than eighty pages are devoted to the hair, considered in its medico-legal aspects. But Botany and general technique receive over three hundred and fifty pages. G. L. G.

3. *Contributions to the Physiology of Growth.*—J. Wortmann (Bot. Zeit., April and May, 1889), describes a method of studying the effects of different media upon the growth of root-hairs, with special reference to the increase or the diminution of turgescence.

Seeds of Lepidium were made to sprout in moist air, with the roots of the germs vertical. As soon as the root-hairs began to appear, the seedlings were placed under a cover glass on a slide and the given liquid added. Under such circumstances the root-hairs grow for several days and the process can be watched at short intervals. Janse has shown that by increasing the turgor in Algæ by means of plasmolytic media, growth can be made to recommence after an arrest. The method of Wortmann is substantially a modification of Janse's and leads to similar conclusions. As an outcome of his studies, Wortmann believes that he has strengthened the theories of Sachs and of de Vries regarding growth; in short, he holds that growth is the result of mechanical turgescence brought about by the interaction of the surroundings and the interior of the vegetable cell. Some of the distortions produced by varying the media, are very remarkable and indicate that the field should be much further explored. G. L. G.

4. *Atlas deutscher Meeresalgen.* Erstes Heft. In Verbindung mit Dr. F. SCHÜTT und P. KUCKUCK bearbeitet von Dr. J. REINKE. Berlin. Paul Parez, 1889. Folio, pp. 34, Pl. XXV.—In his recent work, *Algenflora der westlichen Ostsee deutschen Antheils*, Professor Reinke, to the surprise of most algologists who supposed that little new remained to be discovered on a coast which had been so long known to marine botanists, showed that a remarkable number of new species and even genera, especially of very interesting *Phæosporeæ*, had escaped the notice of previous explorers. In that work, he gave an interesting account of the new species found by him and critical notes on many others, illustrated, however, by only a few wood-cuts. The present work, prepared at the request of the commission for the scientific investigation of the German Marine Waters in the interest of the fisheries, has for its aim the scientific description and illustration of Algæ of which no adequate account exists in other works. The first part includes twenty-nine species and varieties, among them fourteen of the new species discovered by Reinke. The whole work is to be completed in four parts of the size of the present, but the second part will not be published as a whole but will be issued in fasciculi with five plates. Botanists will appreciate the enlightened spirit of the German commission in entrusting to Professor Reinke the preparation of a work, which is not only thoroughly scientific in its method, but also artistically beautiful in its presentation. The lithographic plates, in part colored, are excellently drawn by Schütt and Kuckuck and the accompanying text gives a clear and well condensed description of the species figured without entering into details of structure and nomenclature which were more appropriately given by Professor Reinke in his Algenflora, previously published. Illustrations of the new chlorosporic genera, *Pringsheimia*, *Epicladia* and *Blastophysa* are given, but twenty of the twenty-five plates are devoted to *Phæosporeæ*. Besides representatives of old genera such as *Ectocarpus*, *Scytosiphon*, etc., we have here for

the first time figures of the new genera *Symphoricoccus*, *Halothrix*, *Kjellmania*, *Microspongium* and *Leptonema*. The *Phæosporeæ* evidently are much more rich in generic types than was formerly supposed, and the complication of species in this order has become so great that, even with as excellent descriptions as those in the *Algenflora*, one would be glad to have also good plates as a supplement. Not the least interesting plates to American algologists are those of the much confused and much abused species, *Ralfsia verrucosa* and *R. clavata*, whose characteristics are here well given. We would add that the remarks on those species in the *Algenflora* are especially to be commended. We have only one suggestion to make. Although the full synonymy is given under the species described in the *Algenflora*, we think it would be well to repeat the main points of the synonymy briefly when the same species occur in the *Atlas*. We would congratulate Professor Reinke on the excellence of the part already published and hope that the work in his hands will progress without interruption. W. G. F.

5. *A Monograph of the Horney Sponges;* by ROBERT VON LENDENFELD. 936 pp. 4to, with 50 plates. Published, for the Royal Society, by Trübner & Co., London, 1889.—The author of this elaborate monograph has worked up the science of sponges, and especially that of the Horney Sponges, from its foundation. His studies on the subject began with the Mediterranean species. In 1881 he extended his researches to Australia and afterward continued them in New Zealand and again in eastern Australia; and in these regions, besides making large collections he had placed at his disposal all those gathered in the museums of scientific institutions. Finally, returning to Europe in 1886, the specimens of the British Museum were opened to him. Mr. Lendenfeld has thus been enabled to make a careful study of and re-describe all previously known species, to investigate the anatomy and physiology of the different types in the field, and obtain that comprehensive knowledge of the subject required for a true classification of the sponges.

The number of species described in the work is 348, of which 258 live in Australian seas and 179 or 69·3 per cent of these are confined to those seas. The European coast of the North Atlantic afforded 12 species, the American coast of the same, 56, and the Mediterranean, 44 species. The number of Australian species found in other regions and the percentage relation is given as follows, omitting the Asiatic and American coasts of the Pacific from all of which only 7 species are recorded.

	Total.	Also Australian.	Per cent Australian.
Mediterranean	44	21	47·7
European coast of North Atlantic	12	3	25·0
African coast of Atlantic	10	4	40·0
American coast of North Atlantic	56	29	51·8
American coast of South Atlantic	12	5	41·7
African coast of Indian Ocean	38	22	57·9
N. and E. parts of Indian Ocean	32	16	50·0

It is thus shown that, according to present knowledge, the Horney Sponges of the eastern and northern parts of the Indian Ocean are not so similar to the Australian as those from the remoter African coast of the Indian Ocean and those from the still more remote American coast of the North Atlantic. "The similarity of the Mediterranean and Australian faunas and the dissimilarity of the European Atlantic and Australian are very striking." The species, it is remarked, are typically shallow water forms. The greatest number occur at depths of 20 to 50 meters and only 17 are reported from depths over 100 meters; the greatest depth of any is 750 meters.

The figures of sponges are mostly photo-lithographs taken direct from the types, and are admirable. A large part of the plates are devoted to anatomical dissections, and all are remarkably fine. The bibliography occupies 68 pages. The author adds to his acknowledgments: "Throughout my labors I have had the constant assistance of my wife, and by her the greater number of my microscopic preparations were made."

6. *The Bermuda Islands; a Contribution to the Physical History and Zoology of the Somers Archipelago with an examination of the structure of Coral Reefs.* Researches undertaken under the auspices of the Academy of Natural Sciences of Philadelphia; by ANGELO HEILPRIN. 231 pp. 8vo, with 17 plates illustrating the zoology of the Bermudas, and others, its scenery and geology.—Professor Heilprin has made an attractive volume for the Bermuda traveler, by his vivid descriptions of the islands and of their scenery above the water and beneath it, illustrated by phototypes, and by his account of the coral reefs and discussion of their origin. The scientific reader will find that the subject of the reefs and that of Bermuda zoology is presented by one who fully understands the problems he deals with, and is familiar with much of the life of the seas. His conclusion on the question of the origin of Coral islands he expresses as follows: "If the theory of subsidence cannot, perhaps, be considered to be absolutely demonstrated, it accords best with the facts, and, indeed, may be said to be in substantial harmony with them." The additions to the list of Bermuda species of invertebrates is quite large. The number of known marine mollusca living about the reefs is increased from about 80 to 170 species, all of them excepting less than a dozen, "members of the West Indian or Floridian faunas. Eleven are peculiar to the islands, and are here first described. So again the number of known species of terrestrial mollusks is increased from about 20 to 30; sixteen of all are West Indian, and three are species of East Atlantic islands. Similarly the lists for other departments of invertebrates are much increased, and made valuable by original notes.

To his own notes on the faunas, the author adds chapters on the Actinology of the Bermudas by Professor J. PLAYFAIR MCMURRICH, on the Insects by Dr. P. R. UHLER, on the Spider fauna, by

Dr. G. MARX, on the Myriapods, by Mr. C. H. BOLLMAN, and on the Helicoid Land Mollusks, by Mr. H. A. PILSBRY.

An appendix contains notes on the recent literature of coral reefs, presenting fairly the general course of argument in the several papers or works mentioned.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *British Association.*—The meeting of the British Association for the year was held at Newcastle, commencing on Wednesday, September 11. The address of Professor W. H. FLOWER, the president, was devoted mainly to the subject of the arrangement of museums, but it closes with a few words on evolution which end in the expression of his opinion, that the "principle of natural selection has played a most important part in the production of the present condition of the organic world; that it is a universally acting force continually tending toward the perfection of the individual, of the race, and of the whole living world." The address in the section of Mathematics and Physics was by Capt. W. de W. ABNEY, on the effect of light on matter; in that of Chemistry, by Sir LOWTHIAN BELL, on progress in metallurgy through the aid of chemistry; in that of Geology, by Professor JAMES GEIKIE, on progress in knowledge and theory with respect to the Ice-age; in that of Geography, by Sir F. de WINTON; that of Economic science and Statistics, by Professor F. Y. EDGEWORTH, on mathematical reasoning in political economy; that of Mechanical science, by WM. ANDERSON, on the molecular structure of matter, etc.; that of Biology, by Professor J. S. BURTON SANDERSON, on the mechanism of life in its simpler aspects; that of Anthropology, by Sir WM. TURNER, on the subject of heredity.

For the several addresses in full, the reports of committees, and abstracts of papers read, we refer readers to "Nature," commencing with the number for September 12.

2. *Scientific Papers of Asa Gray. Selected by* CHARLES SPRAGUE SARGENT. In 2 vols. 8vo. of 398 and 504 pp. (Houghton, Mifflin & Co.).—These handsome volumes contain a selection from the reviews, biographical sketches and shorter essays of Dr. Gray, and not from those issued by himself in volume form, nor any of his papers in descriptive botany. For fifty years he was personally identified with the botanical work of the world, and particularly with that of America; and, besides his special labors in the science, he was led by his interest in botany and in his fellow-workers to write reviews of botanical publications as they appeared, biographical sketches, and essays on various topics suggested by his own researches or those of others. These shorter papers, therefore, "furnish the best account of the development of botanical literature during those fifty years." Prof. Sargent states in his Preface that the amount of material he had for selection was overwhelming, the bibliographical notices and reviews

alone numbering eleven hundred. He has performed his duty well, having made a work that will be welcomed by all botanists, and friends of science—a memorial work, as the genial, judicious, truth-loving Dr. Gray is every where brought to mind; a historical work, historical of workers as well as work done; and a repository of views and discussions connected with structural and physiological subjects, the geographical distribution of species, nomenclature, heredity, variation and self-fertilization in plants, and various other topics, always instructive and often entertaining. Many of the papers are very much like talks from their author; and they present him in so many different moods, dependent on the subject in hand, that the volumes serve quite well in place of a biography.

3. *A popular Treatise on the Winds*, comprising the general motions of the atmosphere, monsoons, cyclones, tornadoes, water spouts, hailstorms, etc.; by WILLIAM FERREL, M.A., Ph.D. 505 pp. 8vo. New York, 1889 (John Wiley & Sons).—The great progress that has resulted from the recent systematic study of meteorology is well shown in this excellent volume. The eminent author has himself made important contributions to this advance. It could be safely assumed that a work from his pen would most satisfactorily represent the present state of the science, and this expectation is not disappointed.

The topics presented embrace the general constitution and circulation of the atmosphere, and further those disturbances in the general equilibrium which manifest themselves as cyclones, tornadoes, thunder storms, etc. These subjects are presented in simple, direct language, and with very little mathematical analysis, so as to appeal to a large class of readers who are not in a position to take up the study in accordance with the more profound scientific method.

4. *Sixth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution.* 1884–85, J. W. POWELL, Director. 675 pp. royal 8vo., with many illustrations. This richly illustrated report contains a paper by Mr. Wm. H. Holmes of 175 pages on the ancient art of the Province of Chiriqui, and another, of 50 pages, on a study of the textile art in its relation to the development of form and ornament; a paper by Dr. Franz Boas, 165 pages, on the Central Eskimo; one of over 100 pages, by Mr. Cyrus Thomas on the study of the Maya Codices, and one of 30 pages, by J. O. Dorsey, on Osage traditions.

5. *Elementary Algebra;* by ROBERT GRAHAM. Longmans, Green & Co.—This text-book develops the ordinary subject of Algebra including the Binominal Theorem but not logarithms.

6. *Numbers Universalised, an Advanced Algebra;* by DAVID M. LENSENIG. Part First. D. Appleton & Co.

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ERRATUM.

Pages 323, 324, for Sumpter, read Sumter.